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The

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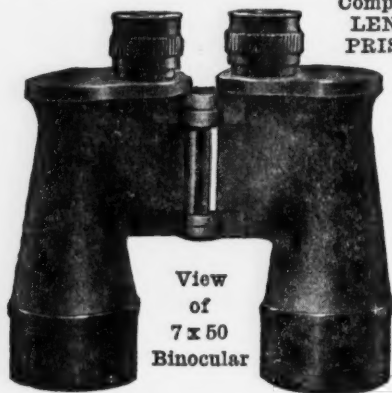
March 1946

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THE SCIENTIFIC MONTHLY

MARCH 1946

JAMES BRYANT CONANT

PRESIDENT OF THE A.A.A.S. FOR 1946

By VANNEVAR BUSH

THE election of James Bryant Conant brings to the presidency of the American Association for the Advancement of Science a leader whose career is convincing demonstration of the effectiveness of scholarship in action. It is no disparagement of Dr. Conant's notable attainments in chemistry and his acumen as a researcher to say that in calling him from his laboratory in 1933 to become its twenty-third president, though it demanded of him great personal sacrifice and though it took from the scientific investigation of photosynthesis a sadly missed pioneering mind, Harvard University did education and the country a great service. For the opportunities and the responsibilities which then became his, he was ready; and as problems in fields far from scholarship and education arose to require his keen analysis and solution, he had already grown to meet them. His university and the nation as a whole are, by consequence, continuing beneficiaries.

Dr. Conant's measure as a statesman of the intellect had been well foreshadowed a number of years before his recent and present preoccupation with the world-relations of the United States as an aftermath of the war. His annual reports as President of Harvard epitomize his driving belief in democratic opportunity in education and his common-sense approach to ways of opening

and stabilizing it. Americans are a report-writing people, and the annual output of such documents is therefore staggering in more ways than one. Without exception, Conant's reports have been oases, broad in conception, lofty in aspiration, sound and practical in application. The philosophy of education which they express came to him as part of his New England birthright. It has been matured and consolidated both by experience and by earnest thinking, and in it as thus developed are to be found the basis for Conant's leadership as a citizen and the explanation of the unselfishness with which he has devoted himself to public service.

His recent activity as adviser to the Secretary of State in negotiations looking toward international control of atomic energy illustrates Conant's scholarship in action for the establishment and stabilization of peace among nations. The convictions of the necessity for world concord which have impelled his work for peace are the same convictions which made him a militant interventionist as long as seven years ago and brought from him the first American demand for "unconditional surrender," only two months after the day of Pearl Harbor.

But both in his firm insistence that the United States must carry determined war to an unrelenting conclusion against



JAMES BRYANT CONANT

Photograph by Bachrach

the Axis and in his unstinted collaboration in the endeavor to prevent the recurrence of such a grim necessity, Conant is no summer soldier or sunshine patriot. He knows the harsh bargain that war drives with those who slacken their grip on peace and therefore have to buy it back with blood and treasure. Conant learned this lesson in the first World War, when as a twenty-five-year-old major in the Chemical Warfare Service he directed top-secret work on gas warfare. It fortified him for the heavy demands which the second World War was to impose upon him.

Conant's active concern over the war was voiced as early as 1939, and with the formation of the National Defense Research Committee in June 1940, he went to work in dead earnest, taking responsibility for NDRC's development of his World War I subject—chemical warfare. As the country's war research expanded, the Office of Scientific Research and Development came into being in 1941, and Conant took on the chairmanship of NDRC, which became the central agency in OSRD military research and development, involving the expenditure of many millions of public funds. His contribution to the success of American arms through this work is itself incalculable.

Through most of this period, however, Conant was at the heart of the great program of research, engineering, and administration that brought nuclear fission to practical utility. His manifold other accomplishments through his post in NDRC are equaled in significance by what he did in co-ordinating and spurring the project which reached achievement in New Mexico last July and which a month later put an end to the war. The Smyth report gives in stark outline the history of his participation in this work—the successive reorganizations from the original Uranium Committee

through a series of units to the OSRD S-1 Executive Committee under his chairmanship, as well as his contributions to the formation of policy through the so-called Top Policy Group formed in 1941 and the Military Policy Committee which came into being after the prosecution of the work had reached such a stage that the Manhattan District was created and the Army began to assume operating responsibility for the development. Unreported but vivid in the memories of his colleagues—military and civilian—through these years is the record of his rare combination of hard-headed analysis and tactful collaboration in problems that were ever new, ever changing, and charged with vital importance to the country and to men everywhere.

In Conant's personal history up to the war, Harvard is the dominant note. For all but sixteen of his fifty-two years—he was born in Dorchester, Mass., March 26, 1893—as student, teacher, or administrator, he has been in and of it. Prepared at the Roxbury Latin School, he was graduated as a Bachelor of Arts in 1913, and received the Ph.D. and became an instructor in chemistry in 1916. He advanced through the grades to become Sheldon Emery Professor of Organic Chemistry in 1929 and chairman of the department in 1931, holding these posts at the time of his election to the presidency of the University on June 21, 1933. As a chemist, he has worked on the quantitative study of organic reactions, on hemoglobin, on free radicals and superacid solutions, and on chlorophyll. In 1932, Columbia awarded him its Chandler Medal for achievement in chemical science, the subject of his paper being "Equilibria and Rates of Some Organic Reactions." In the same year, the William H. Nichols Medal of the New York Section of the American Chemical Society came to him for his

work on chlorophyll. The gold medal of the American Institute of Chemists, for "noteworthy and outstanding service to the science of chemistry or the profession of chemistry in America" was awarded him in 1934.

Married in 1916 to Grace Thayer Richards, Conant has two sons. His personal interests are in simple things: He is a good fisherman, and used to go skiing on occasion—in fact, he once cracked up a shoulder on a tricky slope, to the dismay of the Harvard Corporation. Thanks to the fact that one of his grandmothers was born in 1800, he is a member of the executive committee of the extremely select Association of Grandchildren of the Eighteenth Century. Good talk is one of his great delights, and he is adept both at drawing out the other fellow and at keeping up his end of a conversation.

He comes to the presidency of the Association at a high point in a distinguished career. With war responsi-

bilities drawing to a close, he will bring to the problems of scholarship and education the vigor of his basic belief in democratic opportunity and the added strength of convictions matured by reflection and observation during this period of stress. It is notable that, with all the pressure of recent years, Conant has made time for thinking and writing on the essential questions of the relation of the scholar and the state, and the ends and means of learning. Moreover, it was he who in the midst of unprecedented and taxing demands, set in motion and followed in operation the fundamental study of educational philosophy and policy which is embodied in the report of the Harvard Committee, *General Education in a Free Society*. The advance of the Association toward its objectives will be spurred by his influence, as other efforts at resolving the perplexities of bettering the minds and therefore the lives of the people have gained by his guidance.

THE SUN MAKES THE WEATHER

I. MEASURING SOLAR VARIATION

By C. G. ABBOT

RESEARCH ASSOCIATE, SMITHSONIAN INSTITUTION

AFTER his term as Speaker of the House of Representatives, "Uncle Joe" Cannon of Illinois came back to the Appropriations Committee, of which he had been a distinguished member. Representative Fitzgerald of Brooklyn was the new chairman of the Committee.

In the midst of the Smithsonian hearing the chairman said: "The next item is the Astrophysical Observatory. What is it? What does it do? What good is it?"

"Mr. Abbot will speak to that, Mr. Chairman," said Secretary Walcott.

I had just begun to tell of our work when the chairman was called from the room. Said Representative Sherley of Kentucky: "This is rather interesting, but I think Fitz would have his troubles if he tried to explain it on the floor of the House."

Old Uncle Joe was walking to and fro, with his cigar tilted high, as the cartoonists liked to draw him. He stopped opposite Sherley and said: "No, Sherley. I recollect when old Professor Langley came to me and said, 'Mr. Cannon, I need \$4,000 for the Astrophysical Observatory in order to investigate the infrared spectrum of the sun with the bolometer.' 'My God, Professor,' said I, 'Can't you abolish it?' But no, Sherley, one may forget about the stars that are so far away it takes light a thousand years to come from 'em, and if they were all abolished tonight our great-grandchildren would never know the difference—we can forget the stars, but everything hangs on the sun, Sherley, and it ought to be investigated, and I think this appropriation is all right."

We got the appropriation.

For 25 years we have worked on three problems. How much does the sun's heat that warms the earth change from time to time? How much do these heat changes affect the weather? Can sun-produced weather changes be predicted?

To solve these problems my colleagues of the Smithsonian Institution and I have observed the heat of the sun's rays at 12 widely separated stations ranging from sea level to 14,500 feet in altitude, and with automatic apparatus we have even measured sun heat from balloons at 15 miles' elevation. Many months of daily observation have been spent at each of the 12 stations, except Mount Whitney, and 3 of them, Mount Wilson and Table Mountain in California, and Montezuma in Chile, have been occupied for a score or more of years.

Our daily observations, aimed to re-



CHARLES GREELEY ABBOT

BY C. K. BERRYMAN, WELL-KNOWN CARTOONIST.

cord the variation of the sun, begin at the stations soon after sunrise and last till about 10 A.M. After that the plates must be developed and measured. Then come the computations, which often last far into the night. The best of our stations are on mountaintops in desert lands, thousands of miles from home, where there is neither bird nor beast, plant nor insect, nor any human neighbors within many miles, and where rain rarely falls. At Montezuma, Chile, all food, and even water, is hauled from 12 miles away by the observers themselves.

As a result of this long and strenuous investigation, I believe I know the answers to our three questions.

When I speak of the sun's heat that warms the earth I mean the heat equivalent of those rays which fly through space from the sun to us in 8 minutes, moving 186,000 miles each second. Some of them we can see and we call them white light. Sir Isaac Newton proved that white light is really a combination of the colors of the rainbow. Beyond the violet lie rays invisible to us, which we call ultraviolet; beyond the red are invisible rays which we call infrared. All of these, when they enter substances, such as clouds, water, or soil, produce heat. Thus the sun's rays keep our earth warm enough to live on, and if indeed they alter, the weather should alter too. Radio rays are similar to infrared sun-rays except that their wave lengths are much greater. So far as we know, they are not contained in sunrays.

When I ask if the sun's heat that warms the earth changes from time to time, I mean before it reaches our atmosphere, while yet altogether in free space in the form of waves. In other words, is the sun a variable star, as many other stars are? The answer is yes.

WHEN we began these studies about 40 years ago, scientists did not know within wide limits how much heat the sun sends

the earth. The best textbook of meteorology published about 1900 gave values ranging through 250 percent without offering a preference. Both in theory and in practice the subject was chaotic. It is now almost universally admitted that the Smithsonian observations and publications have established that outside the atmosphere the sun's heat is known within 1 percent. We call it the "solar constant of radiation." If there could be a cube of water 1 centimeter (.4 inch) on edge, so black that it would fully absorb sunrays, situated on the moon, where there is no atmosphere, in March when the sun is at its mean distance, and exposed with its surface at right angles to the sunbeam, then its temperature would rise 1.94°C . (3.49°F .) in 1 minute of time.

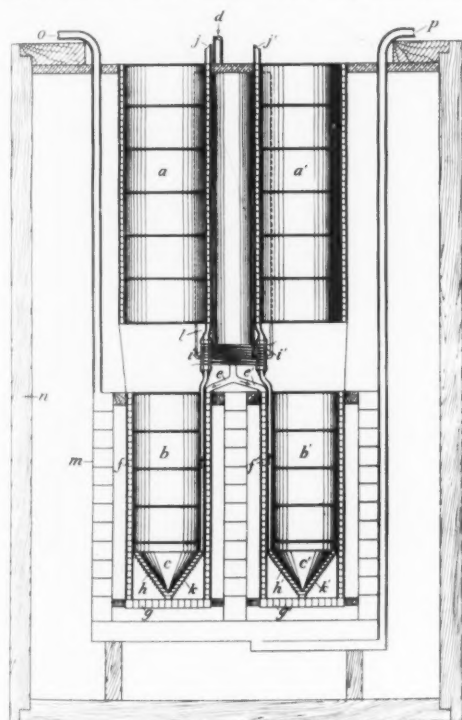
Though this is called "the solar constant," many thousands of our measurements of it show that it is subject to small variations, seldom exceeding 1 percent, but on rare occasions reaching a range of 2 or even perhaps 3 percent. It varies from day to day because the sun rotates on its axis in about 27 days and is not equally bright all over. Thus unequally bright areas face us from time to time. It also varies over the months and years, but we are not yet sure that we know why. Some of our critics doubt if our measurements of the solar constant are accurate enough to show variations as small as 1 percent or less. They argue that even if our ground measurements have sufficient accuracy, still our observing stations on the earth's surface have above them an ocean of air, charged with ozone, water vapor, carbon dioxide, dust, and clouds, tending to throw the estimates into error. Can such a formidable obstacle be overcome?

First of all, we have to measure the sun's heat at ground level. For this purpose I perfected the "silver-disk pyrheliometer" about 30 years ago. Compared to a sewing machine or an

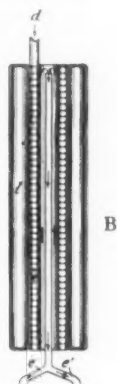
automobile it is a very simple instrument—merely a disk of silver about the diameter of a half dollar and a quarter of an inch thick. It lies, flat side up, at the bottom of a brass tube 15 inches long, through which the sunrays pass. The exposed front of the silver disk is painted dead black to absorb the sunrays. A thermometer bulb is inserted radially in a hole in the disk. The observer merely measures how fast the thermometer rises because of the sun's heating the blackened disk. Nearly 100 of these silver-disk pyrheliometers have been built and standardized at the Smithsonian Institution under my direction and have been furnished at cost to observatories and experimenters on all continents. Published accounts by Australian and Argentinian observers, as well as our own experience, show that these instruments

can measure to an accuracy of .25 per cent, and that they have retained their accuracy unimpaired for over a quarter of a century.

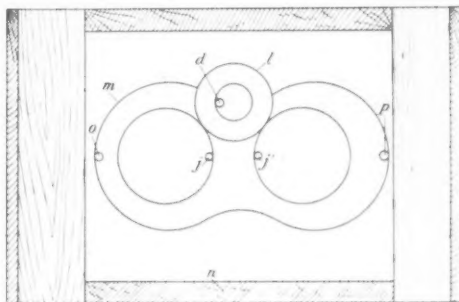
To express our results in accepted heat units, however, we had to devise a primary standard pyrheliometer, with which to compare these silver-disk field instruments. For this purpose I invented the "water-flow pyrheliometer." Everyone has noticed how absolutely dark an unlighted chamber seems, and how all of its objects near the back wall fade into invisibility when viewed from outside through a small opening. In fact, such a dark chamber is a complete absorber, better than the blackest paint. With this in mind I used a dead-black tube to receive and absorb the sun's rays. Its hollow walls have a spiral channel from end to end within their thickness.



A



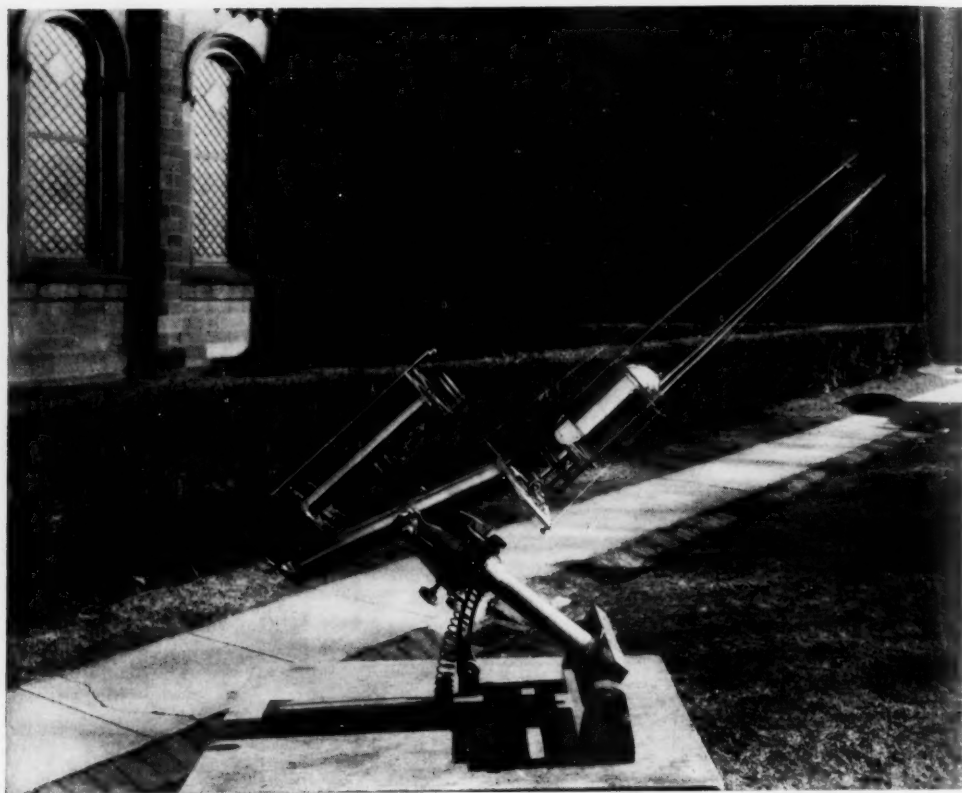
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C

THE SMITHSONIAN'S WATER-FLOW PYRHELIOMETER

USED TO STANDARDIZE INSTRUMENTS FOR MEASURING THE INTENSITY OF THE RADIATION OF THE SUN.



TWO PYRHELIOMETERS AND A PYRANOMETER

Through this channel a steady current of water flows at a measured rate to carry off the sun's heat as fast as received. By a delicate electrical thermometer we can measure the rise of temperature of the water caused by the sun's heat. The area of the front orifice through which sunrays pass into the instrument is accurately known. Thus we can measure how much a given weight of water is heated by the sunrays received in one minute through a known aperture, and fully absorbed. To make sure we are right, we have a coil of wire within the chamber near its back end, through which a measured current of electricity may be passed. This will produce a known amount of heat, which may be measured by the flowing water method as if it were sun's heat. Our experiments

show none of the electrical heat escapes measurement. Hence we infer that is so with solar heat also. Such is our standard water-flow pyr heliometer. At a European conference, many years ago, Dr. Gustav Hellmann, then chief of the German Meteorological Service, said publicly: "There is but one standard pyr heliometer in the world. It is located at the Astrophysical Observatory of the Smithsonian Institution."

Dr. Langley once said in substance: "The difficulty of measuring sunrays accurately at the ground is indeed very great, but the difficulty of measuring their loss in the atmosphere is perhaps insuperable." So, though we had mastered the first difficulty, we had this discouraging outlook to face as we turned to the second branch of our problem.

I shall not try to explain the theory and practice of estimating the transparency of the atmosphere for sunrays. Readers who wish to study it may consult Volumes 2 and 6 of the *Annals of the Astrophysical Observatory*. It will suffice here to say that we are required to locate our observing stations on mountains in cloudless deserts; that we take advantage of the fact that when the sun advances from morning towards noon the path of his rays in our atmosphere steadily diminishes; that we necessarily measure separately at about forty different places in the color spectrum, from far beyond the violet to far beyond the red; and that we use the Langley bolometer, which is an electrical thermometer so sensitive that it measures changes of 0.000,001 degree in temperature.

It is easy to believe that when we use an electrical thermometer sensitive to a millionth of a degree, we have to do it in very steady surroundings. The late Edgar Moore, of Los Angeles, suggested



MONTEZUMA STATION, CHILE

THE ORIGINAL DWELLING, ABOVE, WAS LATER REPLACED BY A REINFORCED CONCRETE STRUCTURE. THE INSTRUMENTS ARE ON THE MOUNTAINTOP.



THE NEW DWELLING AT MONTEZUMA, CHILE

THIS BUILDING WAS CONSTRUCTED TO WITHSTAND EARTHQUAKES. ITS HEAVY REINFORCED CONCRETE FOUNDATION RESTS MOSTLY UPON SOLID ROCK AND ITS BRICK WALLS CONTAIN THICK WIRES.



MOUNT MONTEZUMA, CHILE
APPARATUS AND OBSERVING TUNNEL USED EVERY
DAY FOR MEASUREMENT OF THE SUN'S RADIATION.

to me many years ago that we could insure a much more constant temperature around our instruments by mounting them within a tunnel, dug horizontally far back into the mountain. The sun-beam to be measured is reflected into the tunnel and made stationary in a north-south direction by controlling the mirrors with a suitable mechanism. We

read in the Bible that Joshua made the sun stand still in the heavens. We cannot do that, but we do make its rays stand still in our measuring tunnels. Moore's scheme has gone far to insure to our results their high degree of accuracy.

You may be sure that I was proud when the late Professor Turner of Oxford in the year 1908, reviewing Volume 2 of the *Annals of the Astrophysical Observatory*, wrote: "Mr. Abbot has shown that he is measuring something definite, for he has detected an annual diminution of 3.5 percent from August to October, due to our greater distance from the sun." But the results published 32 years later in Volume 6 of the *Annals* disclose and measure changes in the sun's heat five times smaller than those for which Turner praised us.

On every promising day, for over 20 years at our 3 mountain observatories, we have made these studies of the sun's heat as it is outside our atmosphere. These stations are thousands of miles apart, one in the Southern, two in the Northern Hemisphere. Their results



VIEW FROM THE NEW DWELLING AT MONTEZUMA



LABORATORY, SMITHSONIAN OBSERVATORY, TABLE MOUNTAIN, CALIFORNIA

agree beautifully, and all tell the same story, that the sun is a variable star.

This program is still in progress, and thus far has involved making about 20,000 measurements of the solar constant. Its results, from 1920 to 1939, are contained in very long tables of figures, illustrated by diagrams, occupying the last half of Volume 6 of the *Annals of the Astrophysical Observatory*. They have required great skill and devotion on the part of our observers in the field. Not only do they live like hermits, work like laborers, and observe like top-notch laboratory men, but they spend many hours every day measuring plates and computing the preliminary results. These they send to Washington, where four highly-skilled people go over every observation meticulously, compare the results, detect and cure errors, and apply

little corrections only possible to determine from making use of thousands of days viewed as a whole. The preparation of Volume 6 of the *Annals* required almost four years of solid work by the workers at Washington, besides what was done in 20 years by observers in the field.

Some years ago we asked permission of the U. S. Civil Service Commission to appoint a certain young college graduate, whom we had specially trained for two years, to be in charge of the station at Montezuma. We described the place, the duties, the isolation. We told of the use of instruments to measure the sun's heat to 0.000,001 degree, and the necessity of being able to rebuild them on the spot if destroyed by earthquake. Such repairs involve the hanging of a carefully adjusted magnetic device, hardly heavier than a hair, upon a thread of



COMPUTING RESULTS AT TABLE MOUNTAIN

quartz crystal too fine to be seen with the naked eye. We mentioned the long and intricate calculations to be made each day, the early rising and long hours, the personal hauling of supplies—even of water—and the necessity for tact to carry on without friction, both in the observatory family and among the people of the country. We suggested that unless the Commission felt it indispensable to hold a public examination for the place, we be permitted to appoint the young man whose qualifications had been proved by two years' experience under our immediate supervision. The Commission replied, in effect, that as angels from heaven were unlikely to apply, they felt it futile to insist on a public examination.

Two types of solar variation are disclosed in Volume 6 of the *Annals*, short-interval and long-period. No less than 500 well-marked cases were discovered

up to the end of 1939 in which the sun's heat increased, or, on the other hand, decreased in periods of 3 or 4 days each, by amounts ranging from .5 percent up to 2 percent. These short-interval solar variations are probably caused, as I have suggested, by the sun's rotation bringing unequally bright areas of the sun's surface to face the earth. This type of solar variation is very unequally distributed in the colors of the spectrum. It amounts to almost nothing for red and infrared rays, but increases steadily towards the green, blue, violet, and ultraviolet. The sun's variation in ultraviolet rays is at least six times as great in percentage as the variation of the solar constant itself, which of course comprises all kinds of solar radiation.

As is customary in many kinds of long-continued programs of observation, we give in Volume 6 of the *Annals* the average solar constant values for each 10 days, for each month, and for each year.

In this way the day-to-day fluctuations are smoothed away. But as we scan the whole 20 years of observation we see in these smoothed values many cases of rise and fall in the sun's output of radiation. The fluctuations are seldom as great as 1 percent in their range. Yet depending, as each of these average values does, on many days of observation, even these small fluctuations are significant. Indeed they are not all so small. There is one notable case in the years 1922 to 1924 when the sun's heat fell off rapidly by 2 percent, and then gradually recovered itself. I shall show later that this extraordinary case was accompanied by unusual weather in those years, and I shall point out the probability that we are now in the throes of a similar condition.

To the casual glance, the sun's variation, indicated by monthly averages from

above middle *C*, its tone, if analyzed, is found to consist not only of the fundamental *E*, but of its octave, and of many higher tones called harmonics. These harmonics are all closely related mathematically in their periods of vibration to the time of vibration of fundamental *E*. The difference between the sound of the violin and that of the trumpet depends on which of these harmonics are present when fundamental *E* is sounded.

The sun's long-interval variation is similarly made up of different regular waves, or periods of change. All of them are almost exactly integral fractions of 273 months, or 22½ years, which, in our musical analogy, we can regard as the fundamental. Our best knowledge indicates, however, that there are slight deviations from exact integral relationship with the "harmonics," as shown in Table 1.

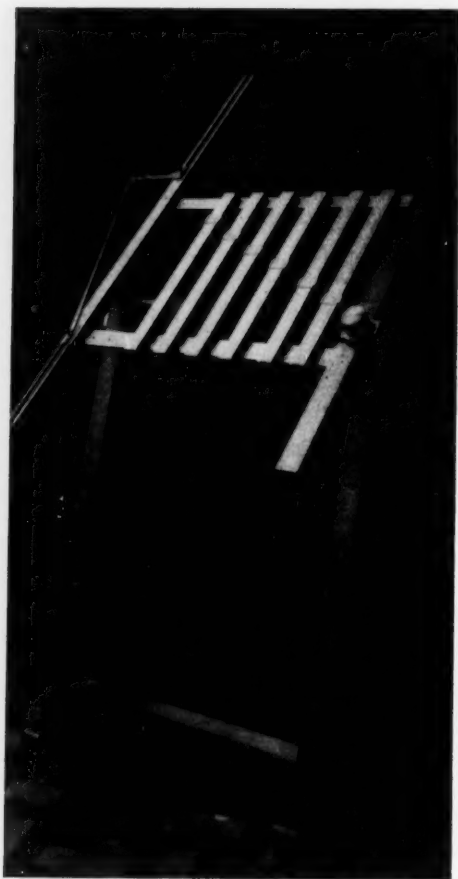
TABLE 1. RELATIONSHIPS OF 14 PERIODS OF SOLAR RADIATION

Solar periods															
observed	273	91	68	54	45.25	39.5	34	30.33	25.33	21	11.87	11.29	9.79	8.12	
Fractions of															
273 months	1/1	1/3	1/4	1/5	1/6	1/7	1/8	1/9	1/11	1/13	1/23	1/24	1/28	1/34	
Computed															
periods	273	91	68.2	54.6	45.5	39	34.1	30.33	24.8	21	11.87	11.37	9.75	8.03	

1920 to 1939, seems altogether irregular, haphazard, and not subject to rule at all. But a prolonged and careful analysis has shown that it is really a complex of as many as 16 regular, simple, periodic terms. We have all watched the great waves sweep in toward the ocean beaches, bearing on their surfaces many little waves. The whole agitation of the water is a complex of big and little waves advancing simultaneously. Somewhat similar is the long-interval variation of the sun with its complex structure of simultaneously active long and short periods. A still more exact parallel is found in the sounds of a violin or trumpet. If one of these instruments sounds the tone *E*

If you ask why the particular periodic variations of Table 1 occur simultaneously, which added together make up almost completely the fluctuations in the sun's output of radiation, I cannot certainly tell. The most likely explanation, however, involves sunspots.

When Galileo made his famous telescope in the year 1610, besides the 4 moons of Jupiter he saw that the sun's surface had black spots upon it, which marched along and crossed the visible solar disk in about 14 days. This shows that the sun rotates in about 27 days, the spots being half the time hidden behind the sun. Since his time the sunspots have been studied. They have been



A SLIDE RULE COMPUTER

proved to be whirlpools in the gaseous substance of the sun. Their numbers range from almost none, as in 1943, up to many, as will be the case about a year hence. Their numbers, in fact, wax and wane in a cycle of about $11\frac{1}{3}$ years, which is called the "sunspot cycle." For at least the past 150 years these cycles have been alternately strong and weak. So there is really a master sunspot double cycle of nearly 23 years which is also the master cycle in the fluctuation of the sun's heat. My 14 solar heat periods are thus all very close to being integral frac-

tions of the double sunspot cycle. They must, almost surely, be related to sunspots in some very deep-seated way.

But what causes sunspots themselves and their $11\frac{1}{3}$ -year cycle of frequency? A great deal of study has been given by many astronomers and others to the possibility that sunspots are caused by the tidal forces of the planets, several of which are sometimes nearly in the same direction, as viewed from the sun. H. H. Clayton has recently published such a study. He finds that these planetary periods almost coincide with periods of variation in the sun's heat, as published in Volume 6 of the *Annals*.

But I will not pursue this clue further. I will add only that weathermen have found that the $11\frac{1}{3}$ -year sunspot cycle produces corresponding periods in temperature and rainfall. Curiously enough, however, I did not find it a period of fluctuation in the sun's heat, as reported in Volume 6 of the *Annals*, but L. B. Aldrich has recently found a curious evidence there of its presence. It is well-known that sunspots are like machine guns, in that they bombard space, including, of course, the earth, with electric ions. This bombardment is very active at times of maximum numbers of sunspots. It is also well-known that electric ions, which in our atmosphere, besides reflecting radio waves around the earth so that we get programs from great distances, in addition act as centers of condensation for the water vapor of the atmosphere and so promote cloudiness, and doubtless also rain. Clouds, of course, also alter temperatures. So in this way, the $11\frac{1}{3}$ -year sunspot cycle becomes a weather cycle and must be added to our group of fourteen. There are thus at least 15 solar periods, likely to be of some importance for weather.

(To be concluded)

SUNSHINE AND THE ATOMIC BOMB

By WILLIAM T. SKILLING

TO COMPARE the atomic bomb with anything so gentle as sunshine *seems* absurd. Why not use a thunderstorm as an illustration if we are seeking one in nature?

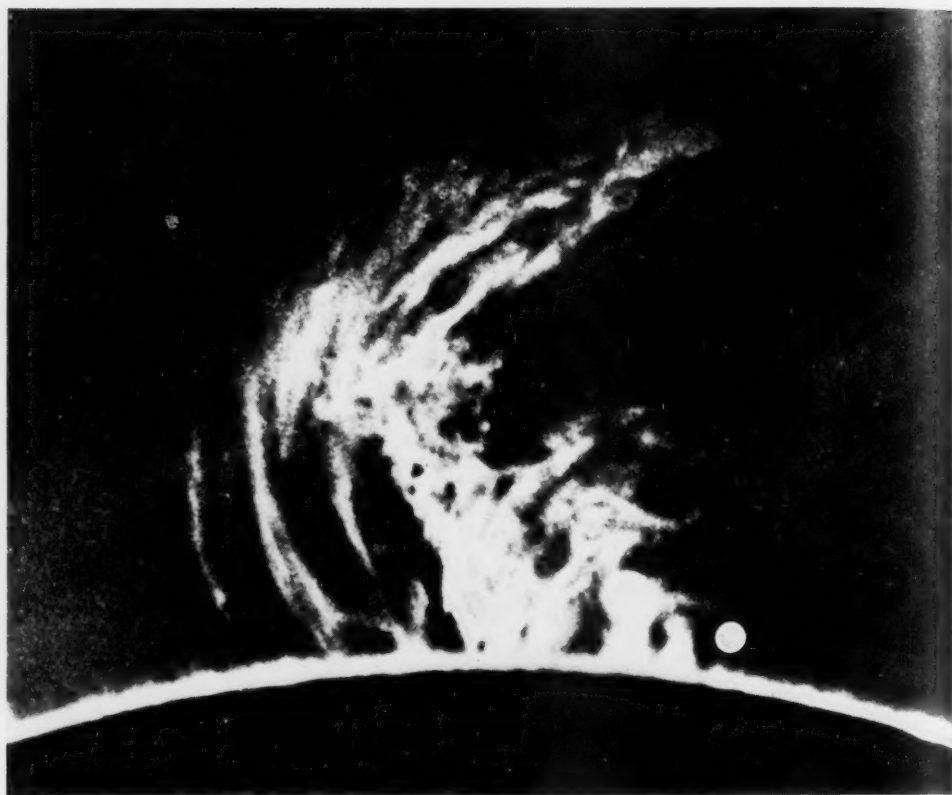
But there are several arguments in favor of our title. In the first place, the actual amount of energy set free in the most violent lightning flash is small compared with the sun's heat energy falling on the earth, the equivalent of 1.5 horsepower per square yard of surface. If all the heat of sunshine could be converted into work, the total work would be greater than could be done by all the horses that would have room to stand comfortably on the earth's surface. A bolt of lightning may be very destructive instantaneously over a small area. Its duration is so brief, however, that when its force is spent, and its energy has been measured as well as scientists can measure so sudden a phenomenon, it is found that the total work that can be done by this much electricity is no more than could be done by the warmth falling on a square yard of the earth's surface in a day of bright summer sunshine.

Back of the power of the great expanding wave of heated air that seared and blasted to bits the city of Hiroshima there was an amount of energy let loose within the bomb greater than had ever been released on earth before from so concentrated a source. The reason for this lies in the fact that the detonator of the bomb opened a tap that turned on a flood of nuclear energy always before held back by the "binding force" of the nucleus. Out of the explosive material in the bomb came power equal to all that could have been generated by burning nearly 3,000,000 times its weight of coal. And even in this explosion only

1/1,000 part of the total nuclear energy of the atom was released!

We have seen photographs of the vast cloud that shot up to a height of 40,000 feet at the explosion. But this would seem like a toy demonstration of force if compared with similar outbursts on the surface of the sun (Fig. 1). The tremendous inner energy of the sun keeps its surface constantly boiling up with great bubbles of white-hot gas called "rice grains," or "granules" (Fig. 2), each as large as any of the states in the Union. Sometimes there are ejected immense cloudlike masses of gas called "prominences." These are often as extensive at their base as the size of the earth, and usually rise to a height of some 30,000 miles, or may go on up to 500,000 miles, with velocities ranging from a few miles to more than 100 miles a *second*. The energy back of all such activities on the sun is from a source similar to that from which comes the power of the atomic bomb.

The fundamental quality of the bomb that links it with the sun rather than with a lightning flash or an explosion of TNT is that its energy comes from changes in the deep interior of the atom, not from its surface. Electrical energy, including lightning, comes from the loose, outer electrons of the atom. The sun's heat and the force that explodes the bomb come from the dense inner core of the atom, called the nucleus, which occupies no more than 1/10,000 part of the atom's size. Lightning and fire both result from a reshuffling of the electrons that circulate around the nucleus as planets go around the sun. The electrons in no case constitute more than 1/1,800 of the atom's weight, and their



Mount Wilson Observatory

FIG. 1. A SOLAR PROMINENCE, 140,000 MILES HIGH

THE RELATIVE SIZES OF SUN AND EARTH ARE INDICATED BY THE SUN'S CURVED SURFACE AND THE WHITE DISK, REPRESENTING THE EARTH, WHICH APPEARS AT THE RIGHT OF THE SOLAR PROMINENCE.

ability to produce energy is a far smaller proportion than that in comparison with the energy that might come out of the nucleus with which they are associated.

Nuclear physics is a new science. The earliest successful attempt to make any change in the nucleus dates back only to 1919, when Sir Ernest Rutherford, of England, bombarded nitrogen atoms with particles flying naturally from radium, and changed some of the nitrogen atoms into those of oxygen. Since then artificial means have gradually been developed for speeding up the atomic particles to such velocities that nuclei struck by these flying missiles are broken. Electricity and magnetism are employed to give speed to the particles

and to guide them against their target. These methods have reached their highest point of success in the atom-smashing cyclotron of Ernest Lawrence, at the University of California.

With the aid of an instrument called a mass spectrograph, experimenters found that when the nucleus of one atom was changed into that of another the weight of the newly formed atom was sometimes less than the total weight of the parts from which it was made. Two and two did not make four, but something less than four. The long-standing law of conservation of mass went out of date. In its place a new law emerged, namely, that if matter thus seems to be destroyed, enough energy is

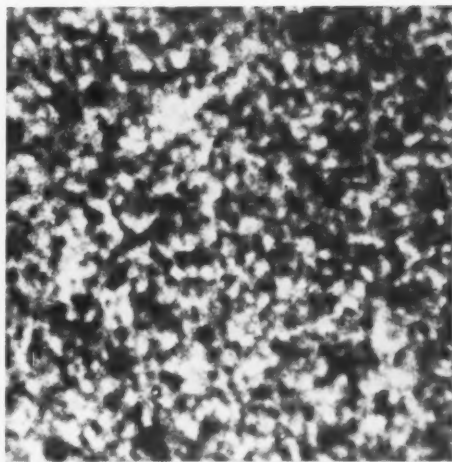
produced in the process exactly to make up for the matter lost.

Long before this could be demonstrated experimentally Einstein had, in 1905, announced on theoretical grounds that if a mass of matter could be converted into energy, the energy produced must be equal to the mass destroyed times the square of the velocity of light, or $E = mc^2$, where E is the energy set free, m the mass of matter lost, and c the velocity of light. (Ergs of energy, grams of mass, and centimeters per second are the units employed in the equation.) This equation forms the basis for all work involving atomic changes, such as work having to do with the atomic bomb.

Although it was not until the end of 1942 that scientists learned how to handle the atom in such a way as to liberate more energy than they had to put into the process of generating it, they had long before settled definitely upon the theory that atomic changes, accompanied with loss of weight, must be the source of the sun's heat. Thus the faint hope was held out to them that they might sometime devise a terrestrial method of imitating solar efficiency.

During the twenties and thirties there waged a lively discussion as to whether in the sun the whole of matter is being annihilated as such, and energy substituted for it, or whether certain atomic changes may be proceeding steadily, induced by such a temperature as the sun is known to have, which would *reduce* the weight of the reacting substances enough to furnish sufficient heat.

Sir Arthur Eddington leaned toward the annihilation theory for a time; it seemed reasonable in view of the supposition that matter is essentially electrical in nature. Eddington reasoned that if a proton, the nucleus of the hydrogen atom, is nothing but a charge of *positive* electricity, and if an electron is an equal *negative* charge, the two chancing to



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FIG. 2. SURFACE OF THE SUN¹
MAGNIFIED 700 TIMES TO SHOW THE GRANULES.

meet in a head-on collision might come into such close contact as to neutralize each other. As far as matter is concerned, this would be annihilation; but in place of the matter there would appear radiant energy, $E = mc^2$, which would be in the form of exceedingly short and energetic electromagnetic waves, very much shorter and more penetrating than X-rays.

The fly in the ointment of this theory was that physicists had never observed any cases of annihilation even on a small scale in the laboratory. In support of the alternate theory of partial loss of mass, there had been observed atomic changes in which the newly formed products weighed less than the sum of the separate parts.

Since the element helium is composed of atoms that weigh *almost* exactly 4 times as much as an atom of hydrogen, it could be very naturally assumed that an atom of helium is, indeed, 4 atoms of hydrogen that have in some unknown way been united. For a decade or so the sun's heat has been attributed to

¹ From *Sun, Moon and Stars* by W. T. Skilling and R. S. Richardson, Whittlesey House (McGraw-Hill), 1946.

some such change as this taking place. The atom of helium weighs less than 4 atoms of hydrogen by an amount equal to 1/140 of the hydrogen supposedly forming it, and this lost mass must become energy. The facilities of the nuclear physicist, including the mass spectrograph for accurately weighing atomic particles, have shown that many atomic changes do occur involving loss in mass, but in most cases much less loss than the hypothetical one mentioned above.

Regardless of whether complete or partial destruction of matter is the source of the sun's heat, the amount of such "fuel" needed to replenish the sun's constant loss of heat seems at first a little disturbing. Basing estimates on the fact that at the earth's distance from the sun each square centimeter of cross-section receives 2 gram calories of heat per minute, it is easy to compute how much matter would have to be destroyed to furnish this much radiation in all directions from the sun constantly. The staggering result is that it would take 4,200,000 metric tons (4,620,000 of our ordinary tons) per *second*. But what seems on the face of it so alarming vanishes as a menace, for the mass of the sun is so great that even at this rate of consumption it would be 150,000,000,000 years before 1 percent of its mass would disappear.

By 1939 so much information had been accumulated as a result of laboratory experiment that it became possible to say with a good deal of assurance that quite a number of atomic changes should be possible at the center of the sun, under the influence of its temperature of 20,000,000 C°. Velocities up to 500 miles a second would be induced in atomic particles, and their mutual bombardment would be sufficient to change some kinds of atoms, though not all. In that year H. A. Bethe, of Cornell University, announced a series of possible atomic changes, the sum total of which would

be exactly equal to the very improbable chance of uniting 4 atoms of hydrogen to form 1 of helium. His series, known as the "carbon cycle," is represented in Figure 3. The cycle begins and ends with a carbon atom—hence the word "cycle"—and consists of collisions of 4 hydrogen nuclei with the carbon nucleus and its successive products, and, in addition, 2 products changing radioactively without the necessity of collision, these being unstable particles.

At each of the six steps there is a slight, but differing, loss of mass and a corresponding production of energy. The final product is helium and an atom of carbon like the one with which the cycle began. The net loss of mass is the difference between the weight of 1 helium atom produced and the weight of 4 atoms of hydrogen used up. The total production of energy may be computed step by step; or, for convenience in computing, the six steps may be united in one, going directly from hydrogen to helium—it makes no difference.

Knowing that atomic energy of some sort must be the source of the sun's amazing output of heat has been an incentive and an encouragement for scientists to make every effort to release some of this boundless store for terrestrial use. Simple computations based on Einstein's relativity equation $E = mc^2$ show that if all the energy of a pound of any kind of matter could be released it would be equivalent to a fabulous total that can be expressed as 15,270,000,000 horsepower hours, or 11,400,000,000 kilowatt-hours, or 21,500,000,000,000 gram calories, whichever kind of unit seems preferable to the reader.

The sun, according to Bethe's carbon cycle, uses 1/140 of this total amount of mass in changing hydrogen into helium. The atomic bomb makes use of only 1/1,000 part of the total in changing uranium into the lighter elements, but even this available proportion produces

an amazing output, as may be seen by removing three ciphers from each of the above total quantities. Perhaps the most comprehensible illustration of the amount of energy resulting from the fission of a single pound of uranium 235 or of plutonium is to say that it is equal to the heat produced by the burning of 2,700,000 pounds of coal!

The five years of experimentation that led to the atomic bomb dealt with

entirely different kinds of atoms from those the sun uses, but in the bomb as well as in the sun the process is one of making changes of one atom into another with an accompanying loss of mass and a corresponding liberation of energy. Instead of using the atom of hydrogen, the lightest atom, as the sun appears to do, on the "Manhattan Project," as the great cooperative experiment on releasing atomic energy was, for secrecy,

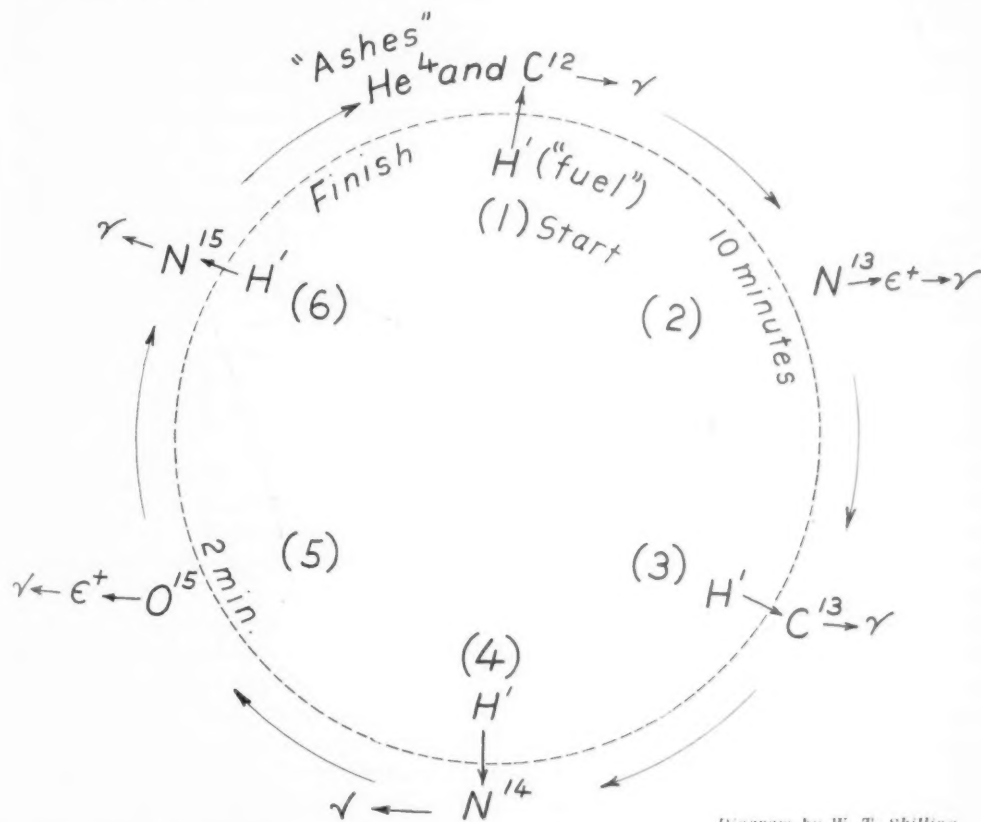


FIG. 3. THE "CARBON CYCLE" OF CHANGES WITHIN THE SUN

ACCORDING TO BETHE, AN ATOM OF HYDROGEN COLLIDES WITH ONE OF CARBON CHANGING IT SUCCESSIVELY INTO VARIOUS ISOTOPES OF NITROGEN, CARBON, AND OXYGEN, AND FINALLY BACK AGAIN INTO ORDINARY CARBON AND AN ATOM OF HELIUM. AT EACH OF THE SIX STEPS OF THE CYCLE ENERGY IN THE FORM OF SHORTER-THAN-X-RAY RADIATION (GAMMA RADIATION) IS EVOLVED. THESE SHORT WAVES OF ENERGY ARE CHANGED TO THE LONGER WAVES OF HEAT AND LIGHT BEFORE REACHING THE SURFACE AND ARE RADIATED OUT FROM THE SUN. THE amount OF ENERGY (HEAT) IS FOUND FROM THE EQUATION $E = mc^2$. THE MASS LOST, m , IS THE DIFFERENCE BETWEEN THE WEIGHT OF THE "FUEL" (HYDROGEN) AND THE "ASHES" (HELIUM). TWO OF THE STEPS (2 AND 5) ARE AUTOMATIC, N^{15} AND O^{15} BEING NATURALLY RADIOACTIVE. THE POSITIVE ELECTRON, e^+ , UNITES IMMEDIATELY WITH A NEGATIVE ELECTRON AND BOTH ARE "DESTROYED"; THAT IS, CONVERTED INTO ENERGY.

called, the workers used the heaviest of all atoms, that of uranium. They selected this because it is naturally so unstable that it constantly changes without any assistance at all into a succession of other atoms until the final product is lead. They chose it also because recent experiments had shown that in uranium there is a small admixture of especially explosive material.

An important incident that helped to touch off the greatest scientific adventure of all time was a visit, in 1939, of Niels Bohr, the father of the modern theory of the atom, to Albert Einstein at Princeton. From Europe he brought word of a remarkable new development in atomic science. During the previous ten years the behavior of atoms had been receiving close attention in various countries by physicists, represented by such well-known persons as Rutherford and Chadwick in England, Enrico Fermi in Italy, Joliot and Irène Curie in France, Otto Hahn in Germany, and Arthur Compton and Ernest Lawrence in the United States. Bohr, on his arrival, reported to his scientific friends that Otto R. Frisch and Lise Meitner, Jewish refugees from Germany, had told him that Frisch had split the atom of uranium by bombarding it with the very penetrating atomic particles called neutrons, and that Otto Hahn, of the physical laboratory in Berlin, had identified one of the products of the "fission" as barium atoms, which weigh somewhat less than half as much as the atom of uranium. (Always before the products of atomic change had been atoms not very different in weight from the atoms from which they were made.) Miss Meitner, of the Berlin laboratory, had computed the enormous energy of such a fission, and suspicion was at once aroused among all scientists who knew of the matter that in this direction lay the looked-for road to the liberation of useful quantities of atomic energy.

Many scientists were now convinced of the possibility of finding a way to secure atomic energy. They took the initiative in awakening an interest in Washington and in military circles. President Roosevelt was quickly responsive and secured a small appropriation to have the work started. Dr. Vannevar Bush, then Chairman of the National Defense Research Committee, led in the organization of the scientific work, and Brigadier General L. R. Groves was later placed in charge of all army activities in connection with the project.

A compelling motive for haste was the knowledge that Germany had at least an even start with us and was trying to add atomic bombs to her other "secret weapons." The work began in 1940 and culminated in 1945, resulting in the building of two new cities, one of 60,000 inhabitants on the Columbia River in Washington, the other in Tennessee. An experimental laboratory, said to have been the best-equipped in the United States, was also established in the desert wilds of New Mexico. The cost of the project amounted to \$2,000,000,000.

The atomic particle called a neutron requires special attention in the discussion of atomic energy, for it has played a most important role in this whole five-year project. Although the neutron is one of the three fundamental particles that form all nature, neutrons were not known as particles until 1932, when they were proved to be such by Chadwick, the English scientist. They had been observed first in Germany, and then experimented with in France, but they had been thought to be "rays," not particles. They are similar in weight to the proton, the nucleus of an atom of hydrogen, but have no electric charge as the proton and the electron have.

This neutral quality gives the neutron power to insinuate itself into the heart of the atom. If charged particles are used for bombarding atoms they are re-

pelled by other similarly charged particles that are their target. But the neutron's lack of charge makes it hard to control; it cannot be speeded up by electric force nor guided by a magnet. To slow it down it must be passed through some light substance—graphite was used in work leading to the bomb. The neutron goes straight ahead until it collides with the nucleus of an atom.

Uranium was made the target for such bombardment by neutrons. Its atoms are the heaviest and most complex, and are naturally radioactive. Uranium, like nearly all elements, is made up of more than one kind of atom; the kind in greatest abundance in uranium has an atomic weight of 238, but one atom in about 140 is a lighter one weighing 235. It is this slightly lighter variety (isotope) of uranium that is "fissionable," that can be exploded. If the U-235 could be separated from the U-238 it would be ideal for use in the bomb, but since these isotopes are so nearly of the same weight, separation on a large scale is almost impossible. Chemical means cannot be used to separate them for, like all isotopes, they are chemically identical.

Fortunately, experiments with uranium both before and after the bomb project was begun led to the knowledge that it is possible to convert U-238 into a different element, as explosive as U-235. This was named plutonium. Being different chemically from uranium, plutonium can be readily separated from the unchanged metal. The making of plutonium is a major part of bomb construction. Great ovens, or "piles," made of graphite bricks, were used to change the huge slugs of uranium metal into plutonium.

Neutrons exist in the nuclei of atoms and are secured for use and given their motion by knocking them out of these nuclei with bombardment of some sort. On a small experimental scale, the most

common way is to mix radium with the metal beryllium and let the particles coming off from the radium drive neutrons at high speed out of the beryllium. The neutrons become the bombarding particles to split uranium 235 and liberate energy.

For large-scale production of atomic energy what is called a "chain reaction" is set up, and uranium itself serves both as a source of neutrons and as a target to be bombarded by its own neutrons.

If uranium were made entirely of U-235 any large quantity of it would explode spontaneously, but in the small concentration of only 1 atom to 140 atoms of the nonexplodable kind, it is only split atom by atom. To get any large quantity of it to undergo fission, the action in the mass must be self-perpetuating. It must be made to go on as a fire does when once lighted; the fire does not go out when the match is removed. Such an action that, once started, is independent of any outside influence is called a "chain reaction."

Two conditions are necessary to set up a self-perpetuating chain reaction in uranium: the high-speed neutrons must be slowed down to a lower rate, called "thermal velocity"; and there must be a large enough mass of uranium so that the flying neutrons will be so well-surrounded with it that not many of the neutrons will escape without hitting a nucleus. Neutrons of moderate speed will make a larger proportion of hits; passing through graphite before coming to uranium slugs slows the neutrons.

The first chain reaction of this sort ever set up was in a graphite pile containing 6 tons of uranium on the campus at the University of Chicago, December 2, 1942. This first feeble "atomic fire" threw off energy at the slow rate of only .5 watt; and, though this rate of action was in a few days raised to 200 watts, this pile would have had to be kept in operation for 70,000 years to yield

enough plutonium to make one bomb. But this experimental pile was soon succeeded by the mammoth plutonium plant on the Columbia River.

The graphite piles are built with openings running through them. In these openings are placed the slugs of uranium, sealed in aluminum cans to keep them dry, as water runs through the pile to prevent overheating. Neutrons strike the uranium atom and are absorbed, thus increasing the weight of the atom, but it is still uranium, for the charge on its nucleus has not been changed. Then the nucleus gives off automatically 2 negative electrons and this does change its charge, raising it by 2, because the loss of negative charge is equivalent to an increase of positive charge. It is now a different element, Number 94, never existing before, and is given the new name, plutonium. Occasionally the uranium, now containing a little plutonium, is removed automatically from the pile and dissolved. Then the new element can be separated chemically from the uranium.

The piles must not work so fast as to explode. Strips of cadmium or steel containing boron are pushed into slots in the pile if action is dangerously fast, for these absorb some of the neutrons, which act like sparks in spreading the "atomic fire." If action is too slow, the strips are drawn out so that more of the neutrons will act upon the uranium. These strips serve a purpose similar to that of the thermostat in an oven. All such operations are either automatic or are performed at a distant instrument board, where the men are well-protected from the harmful rays and particles that are always thrown off in atomic changes.

Naturally the details in regard to the construction of the bomb have been withheld for reasons of security in the report authorized by the War Department and

prepared by Dr. Henry D. Smyth, one of the scientists on the project. One difference between plutonium production and bomb production is that slow-speed neutrons are used in the former and high-speed ones in the latter. This is so that all particles in the bomb will be acted upon before they are blown too far apart for complete explosion to take place.

It is believed that within the bomb the explosive is stored in separate compartments, each portion so small as to be below the "critical mass" necessary for a chain reaction to be set up. Then to detonate the bomb it would be necessary only to bring these separate masses very suddenly together so that the mass will be above the critical quantity required to touch off the chain reaction. The igniting neutron sparks could be furnished by the presence of a little radium and beryllium, or the few neutrons always present in uranium. Even cosmic rays could serve as the igniting spark.

What can be said at this time of the future use of atomic power? Since that day in December 1942, when the first chain reaction was set up among the atoms, it has become evident to scientists that some future use of nature's greatest storehouse of energy is inevitable. When it was found that only a *part* of the liberated energy was necessary to make the process continuous and automatic, it could be easily seen that the *rest* of the power might be harnessed to do some useful work. Though this fraction of the atom's total potential energy that is over and above the fraction necessary for the automatic continuation of the process be ever so small, it becomes a matter of mere technique to increase the available fraction, as has been so abundantly shown since that first feeble display in 1942.

THE LENGTHENED SHADOW OF A MAN AND HIS WIFE—II*

By JAMES G. NEEDHAM

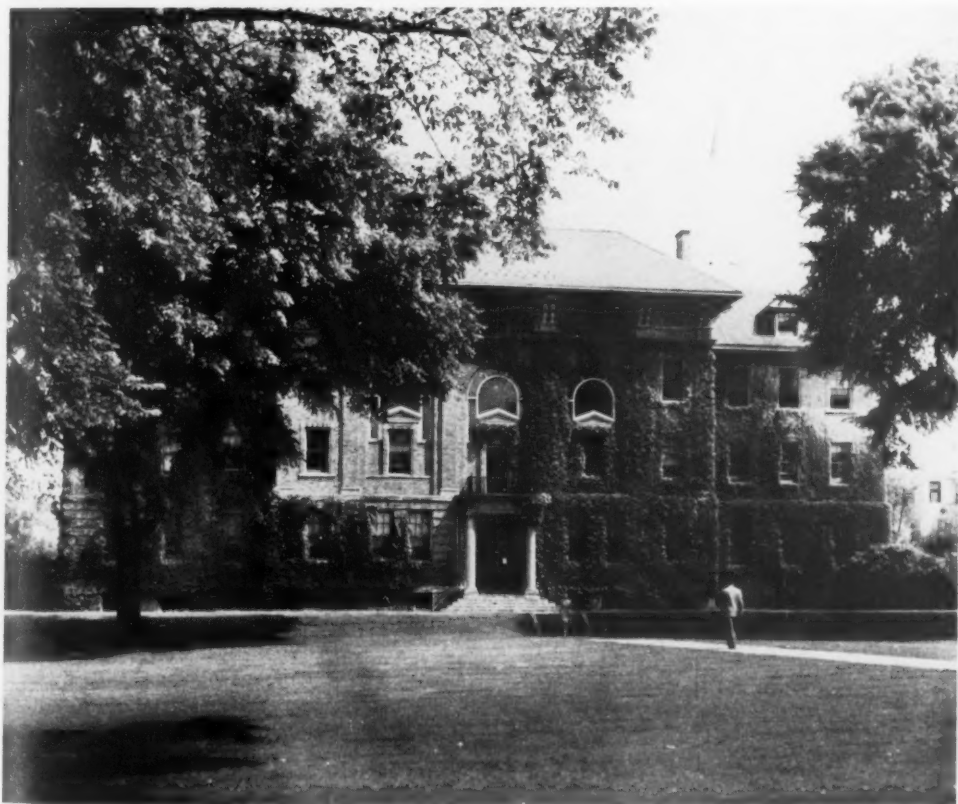
PROFESSOR EMERITUS OF ENTOMOLOGY AND LIMNOLOGY, CORNELL UNIVERSITY

WHILE Professor Comstock, as hereinbefore noted, in addition to teaching large classes, was doing many things for the promotion of interest in entomology, Mrs. Comstock was also doing things on the side. She had married before the completion of her college course, and now she wanted to finish it. She managed to take a few courses in her spare time and to graduate from Cornell in 1885. She entered into the social life of the faculty, where she was a great

favorite. She attended many social functions, taking her husband along when she could get him to go, and understandingly leaving him behind when she couldn't.

She was keeping a diary, which became much more than a record of events. It told a story of high-brow social life in the nineties with a university background. Condensed here and expanded there, it was published as a novel in 1906 under the title *Confessions to a Heathen Idol*, by Marian Lee. A pseu-

* Continued from page 150 of preceding issue.



COMSTOCK HALL, HOME OF CORNELL'S DEPARTMENT OF ENTOMOLOGY

donym was used on the first printing, she said, because it would by some be considered "scandalous" that she should write a novel, and ruinous to her scientific standing. Comstock had read the manuscript, but had offered no assurance of success beyond the qualified endorsement: "For people who want this sort of thing, it is just what they want." A second printing was called for, however, by the public, and that one was made under her own name. No one can read *Confessions* without finding in its kindly philosophy and gentle humor a new understanding of the reason for Mrs. Comstock's social influence.

Mrs. Comstock meanwhile began the study of wood engraving. She was preparing to make suitable illustrations for her husband's projected textbook. For several years, while he was piling up manuscript, she was practicing with her engraving tools, gaining skill with practice and getting what guidance she could in Ithaca. Then she went down to New York City and to Cooper Union for six weeks of special training under the master-artist, wood engraver John P. Davis. Her work there won warm approval, and she was later elected to membership in the American Society of Wood Engravers.

From studying with Davis, she returned to the extended task of preparing the choice engravings that distinguish Comstock's *Manual for the Study of Insects*.

In the year 1889-90 Comstock got his first full-time assistant, Mark Vernon Slingerland. He was a cousin of Mrs. Comstock and came from her home county. He had entered the college as a Freshman two years before, knowing nothing whatever about entomology. Out of curiosity to see what his cousin's bug-chasing husband was like, he attended one of Comstock's lectures. The subject chanced to be the life history of a butterfly. Slingerland had not known before that butterflies come from caterpillars,

or that they have an intermediate pupal stage in their life history. He found the subject of absorbing interest and then and there decided that he wanted to study entomology; and he studied with such zeal and success that he won appointment to an assistantship in that subject while he was still an undergraduate.

Slingerland's interest in insects ran to the applied side. He was quick to see that insect control depends first of all on knowledge of their life histories and habits. He soon made a name for himself by the work that he did in that field. He took over the undergraduate course in economic entomology and a large share of the work on insects in the Agricultural Experiment Station. He was made an instructor in 1890 and an assistant professor in 1907.

Slingerland took to insect photography like a duck to water. When I knew him he almost lived in the insectary with that big, old long-bellows camera. He went to the new insectary with it, stayed with it, all but slept with it; and it became the instrument of his chief contributions to economic entomology.

I shared an office with him in the head house of the insectary in the summer of 1897 and saw him at work. He was out in the greenhouse taking pictures, or out in the field after subjects for more pictures, most of the time. He loved to photograph insects: whole insects; insect eggs; larvae; pupae; insects on their food plants, in their burrows, in their cocoons; singly, in pairs, and in swarms. And he wrote his bulletins around his superb photographs. I never knew another man so wholly devoted to one pursuit.

He set a new standard in entomology for fine photographic illustrations. He built up a collection of lantern slides that was the best in his day, and that will long continue in service.

The next addition to the teaching staff of the Department was made in 1896

when Alexander Dyer MacGillivray was made an instructor.

MacGillivray was a mild-mannered man of very youthful appearance, blue-eyed, slightly stooped, quiet, industrious, and able. He took charge of the laboratory in Comstock's introductory course in general entomology. He was very kind and helpful with students, but also very rigorous in demanding full compliance with the requirements established for that course. Comstock called him "an excellent drillmaster."

While Slingerland was building up a great collection of negatives and lantern slides for the Department, MacGillivray was building up its insect collection. He was a good systematic entomologist; knew the insects of all the orders; collected diligently at every opportunity; pinned, labeled, named, and arranged his specimens with great care. He had a wonderful eye for species and could remember their characteristics and name them at a glance.

Some of his best collecting was done from the freshly gathered material that students brought in from their field trips. He had to supervise the work of students in determining their catch, and whenever an insect appeared that was of a species not represented in the Cornell collection, he spotted it immediately, and begged or traded with the student for it (*he always got it*) and added it to the departmental collection. Thus the collection grew apace.

COMSTOCK chafed a bit under the necessity of teaching entomology during the long winter season, while unable to hold classes in the summertime when the major phenomena of insect life are available for study; so, as soon as he could, he made a shift in his own time schedule. He established a summer term in entomology and took his own vacation during the winter term. His summer course then stood alone. It invited full-time registrants. It soon began to attract

teachers from other schools and colleges. They came to spend their summer vacations devoting full time to the study of entomology under Professor Comstock. Thus the enrollment came to have a considerable admixture of more advanced students and of graduates who were specializing in this field.

The first winter that was freed from teaching by this arrangement gave the Comstocks a chance to go abroad. They spent the winter of 1888 at the University of Leipzig in Germany.

For the next ten winters they were destined to be back in the teaching harness again. Stanford University was born. President David Starr Jordan persuaded the Comstocks to come out to Palo Alto and organize a department of entomology there, where in a milder climate than that of Ithaca insects could be studied in action all year round. It seemed like a good way to spend a vacation. Dr. Jordan was a dear friend, and there were former colleagues from Cornell now members of the Stanford faculty. It was a new and hopeful educational enterprise. A rich and little known insect fauna was waiting to be studied there. So they went to California; and kept on going for ten years.

The second winter they took Dr. Vernon R. Kellogg along as assistant in entomology, and left him there to carry on through the year and later to become head of the very successful department.

The few entomologists who were there had a good time together. Dr. R. W. Doane, who was then a graduate student, once told me some of their doings. He especially treasured the memory of the Sundays when he and Kellogg and Comstock took to their bicycles and pedaled out into the West Hills. They knew a secluded little valley with a running stream, where spring came earlier than elsewhere, where many rare insects were to be found, and where a good farm housewife furnished a hot luncheon, with fried chicken and all the trimmings.

Comstock's load of work was growing heavier. With classes to meet all the year round at Palo Alto and at Ithaca, with supervision of the research work of graduate students, with correspondence, and with frequent faculty duties that he never neglected, he found little time for writing. Each day was too full, and he himself at the end too tired to write effectively, so he decided to write in the morning when his mind was rested. He began the practice of getting up at 4 A.M. each day and going to bed (except when duty otherwise demanded) at 8 P.M. It goes without saying that, on a university campus, few would come into his office between 4 A.M. and breakfast time to interrupt his train of thought.

After the completion of the *Manual for the Study of Insects*, Comstock went to work on a similar treatise on spiders. For his needs far too little was then known about the spiders of the states on our southern border. So he spent a winter in the South, collecting materials for this book. There he had colleagues in the field who helped: noteworthy among them were William Morton Wheeler at Austin, Tex., Harcourt A. Morgan at Baton Rouge, La., and Glenn W. Herrick at State College, Miss.

The work of spiders in spinning their webs fascinated him. He desired that it should be adequately illustrated. He felt that its marvelous detail would be beyond the power of any artist's pencil to portray. So he took to his camera to show it. He kept living spiders and arranged suitable "looms" for their use in spinning in order to get freshly spun and uninjured webs to photograph; he called them "made-to-order" webs. And I can testify from personal knowledge that a prodigious lot of time, painstaking preparation, and patience went into the making of his superb pictures of them. The *Spider Book* was finally published in 1912, and took its place as a standard text and reference book for students of arachnids everywhere.

During the winter of 1907-8 the Comstocks took their first and only sabbatical leave. They spent most of the winter in Italy, Greece, and Egypt. In Europe they visited foreign entomological colleagues: Berlese in Rome; Silvestri in Portici; Simon (arachnidologist) in Paris; Poulton and Hampson in London, and others. In Belgium Comstock was elected to honorary membership in the Société Entomologie de Belgique.

Mrs. Comstock's greatest work still lay ahead. In the agricultural depression of the nineties, when farming had become unprofitable and country youths were flocking to the cities in alarming numbers, wise men were seeking some means of making life on the farm more attractive; educational means, as well as economic. In the hope of interesting future farmers in things of value and of beauty in their rural environment, the New York State Legislature made a first appropriation of \$8,000 for the teaching of nature study in the rural schools, and handed it over to the College of Agriculture to administer.

This was a new kind of agricultural extension. Liberty Hyde Bailey was put in charge of it. He furthered it mightily by writing and speaking, as well as by efficient administration. A series of *Nature Study Leaflets* for use in rural schools was begun at once, and to that series Mrs. Comstock and members of the faculty of the College of Agriculture contributed numbers. "Uncle John" Spencer was drafted from his farm to deal directly with the children in schools. He promoted Junior Naturalist Clubs in the rural schools of the state until he had enrolled at one time more than 30,000 members. But it was Anna Botsford Comstock on whom fell the main task of bringing nature study to the teachers in the schools. To her, more than to any other person, the continuing success of the undertaking is due.

She gave up wood engraving to meet a greater need. She wrote many nature

study leaflets and made the drawings for their illustration. She wrote notebooks for children's use on birds, trees, and familiar plants, and got competent persons to write them on other groups. She discussed nature study at teachers' meetings all over the state, and lectured on it at Cornell and at other universities; at Stanford and Columbia repeatedly; at the University of California in 1906; at the University of Virginia in 1920; and at other educational institutions of various grades.

She organized courses in the teaching of nature study especially for rural schoolteachers at Cornell University. In 1908 she was awarded an assistant professorship by the trustees of Cornell—the first woman to attain that academic standing.

Fine as was her work with a graver, what she did with her pen was even more remarkable. She wrote books; first two small ones that were strictly entomological: *Ways of the Six-Footed* in 1903, and *How to Keep Bees* in 1905. Then she surprised her friends by publishing the hereinbefore mentioned novel, *Confessions to a Heathen Idol*, 1906; then the big two-volume *Handbook of Nature-Study* in 1911, and finally, *The Pet Book* in 1914, and *Trees at Leisure* in 1916.

If asked to name a single one of these for which she would like to be remembered I think she would have chosen the *Handbook of Nature-Study*. That book slowly evolved out of her work on the *Leaflets*, out of her writing for the *Nature Study Review*, which she edited for years, and out of her experience in training teachers of nature study. She consulted her husband, so she once told me, about the desirability of preparing the *Handbook*, and he encouraged her to do it; said he would give her any help he could; said it was much needed, but that she must not expect any financial returns from it, for it probably would never pay printing costs. So it was done

as a labor of love and as a public service. Happily, he was mistaken. Its success was soon assured. It became, and still is, the one most essential reference book of nature study, and it is used around the world wherever nature study is taught in the English language. It is now (1945) in the third printing of the 24th edition, and going strong. Such is Mrs. Comstock's record as a writer.

She was raised in rank to a full professorship of nature study in Cornell University in 1920. She was given an honorary degree, Doctor of Humane Letters, by Hobart College in 1930. She became a member of the board of trustees of William Smith College and of Hobart. She was designated by a poll of the members of the National League of Women Voters, 1923, as one of the "twelve living women who have contributed most in their respective fields to the betterment of the world."

WHEN I first came to Cornell as a graduate student late in the summer of 1896, the summer term had ended, but a few students lingered at the laboratory in White Hall, and Professor Comstock was there with them. He greeted me pleasantly, took me into his office, and proceeded at once to ask me what I had done in my first year's graduate work at Johns Hopkins University. Then he asked about the work in which I had participated earlier that summer at the floating laboratory of the Illinois State Natural History Survey. My first impression was of his alertness to what was going on in fields bordering on his own.

He showed me about the Department. In his office two east windows looked out upon the green of the main quadrangle. Before one window stood his own plain, orderly desk; before the other stood Mrs. Comstock's desk, equipped with light controls and tools for making wood engravings. On a table were prints and proofs; also a typewriter; on the wall, a hand-cranked telephone.

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The Department was up-to-date with Welsbach-mantled gas lights, projection-lantern are light, and a darkroom equipped for making photomicrographs. In the lecture room were long shop-made benches, with backs so aslant that they caused discomfort, but with a writing arm for each student's notebook.

There was but one laboratory for students. It extended across the entire north end of the building. It was equipped with carpenter-shop-made two-drawer tables. There were no desks for students as yet. Graduates and undergraduates shared the laboratory and even some of the tables, for there were more students than there were places to seat them apart and things had to be shared. An unabridged dictionary stood in a corner on a stand of its own. Comstock recommended its constant use as an aid to scholarship.

At that time Comstock, Slingerland, and MacGillivray were the entire staff of the Department. Comstock was giving the lectures in entomology, and in a course in general invertebrate zoology as well. MacGillivray cared for the routine of the laboratory, saw that students were supplied with specimens to be studied, and that they did the work assigned. He spent all his spare time in building up the departmental insect collection. Slingerland was taking over a large share of the experiment station work on insects.

Comstock was deep in his study of the venation of the wings of insects, but the only time he could claim for research was in the early morning hours, before the chimes began to call others to their tasks. By day the door between his office and the laboratory was nearly always open. Undergraduates were not neglected by him; he took a personal interest in the work of every one of them.

There were then but five graduate students in the Department. They sat with the undergraduates in the laboratory, where Professor Comstock came betimes

to see how each student's work was progressing, and to drop hints of excellent things to be learned beyond the bounds of the present laboratory assignment. He did not do any student's work for him, nor did he leave him floundering alone when a mere suggestion of method or material would save his time and temper.

Graduates were expected to take the lectures in the beginning course in entomology along with undergraduates, but even those who had studied entomology elsewhere found that no hardship; it provided further knowledge. Comstock's handling of the subject was a lesson in balanced organization of subject matter, in clearness of presentation, and in simplicity of language. It was said of his lectures that "his winning personal attitude made every student a sympathetic listener."

As my work progressed it was stimulating to have him come in to the laboratory and sit down beside me and say, "Now show me what you have been doing," and to have him express pleasure in every little discovery that seemed to have any significance. It made me eager to go on. It was a treat to go afield with him to see the delight he took in the living world, especially in its insect inhabitants. He loved everything out of doors.

Early in my first year of study with him, after he had invited me to join him in a special study of the developing wings of insects, he said to me one day, "We need to know the tracheation of the wings of the cicada. I planted some 17-year cicada eggs on the campus here sixteen years ago. If they thrived the nymphs hatched from those eggs should now be well-grown. How would you like to help me dig for some of them?" Of course I agreed. So we got pick and shovels and went out to dig together. It was a Saturday afternoon, and the quadrangle was deserted.

There was then a lone hickory tree standing near the south side of the main

quadrangle in front of Boardman Hall. Sixteen years before Comstock had collected twigs from other trees, well-studded with fresh-laid cicada eggs. He had placed the twigs in the boughs of that hickory, where nymphs on hatching from the eggs would fall to the ground. He thought they would burrow down and feed upon the tree's roots. The long developmental period of this species was well-known by reason of the regular seventeen-year recurrence of local broods, but in the field of entomology Comstock liked to be able to speak from personal experience.

We dug and dug. We dug for two hours straight. We dug well down among the hickory roots in several places without finding a single cicada nymph. Finally, as he began to fill holes and to replace sods, he said: "Well, we've had some good exercise. The cicadas didn't like my choice of a home for them. Something in the environment was wrong." As we went back to the laboratory he added: "Some of our laboratory cultures also will fail, but that should not keep us from trying again."

Professor Comstock was not a one-college entomologist. In the middle of my second year of study with him, he made arrangements for my study *in absentia*. For the completion of my doctor's thesis I needed to study Odonata (dragonflies) in a larger collection than Cornell then possessed. He sent me to study in the Hagen collection at the Museum of Comparative Zoology. Besides the advantages to be derived from the use of that wonderful collection, I lived while at Cambridge in happy association with the zoologists at Harvard University: Edward L. Mark, George H. Parker, Charles B. Davenport; paleontologist Tracy Jackson; and the members of the Cambridge Entomological Club. The club in that day met with Dr. Samuel H. Scudder in his private museum. I appreciated those meetings.

Comstock desired that his students

should know something of the men and the methods in other institutions as well as in his own. He considered these men as colleagues rather than as competitors. He entertained no feuds over technicalities; cherished no grievances; was always ready to cooperate. He advised us who studied with him to respect the work and the opinions of others; to stick to facts and not to engage in argumentation. When a piece of research had reached the point of preparation for publication, he required strict attention to the correctness of every statement, saying, "Be sure you are right, and then look again."

Aid and comfort and sound advice he gave me on many occasions. I cannot refrain from mentioning a few additional items of a very personal nature. When I returned to Ithaca to join the Cornell faculty and was looking for a place to live he said to me, "Find a location where your family will have congenial neighbors with like interests."

A little later he said, "If you like golf, the local country club would welcome you to membership. I joined to help a new local enterprise get under way, but I don't play golf often. I find trips afield after insects more interesting. The last time I figured up accounts I found that my golf was costing me \$18 a hole."

When I had been selected by the New York State Museum officials to set up a field station for the study of aquatic insects in the Adirondack Mountains, he said to me, "How fine it is to get paid for doing what you would be doing without pay for fun and by preference."

After receiving my doctor's degree from Cornell University in 1898, I went to Lake Forest College as Professor of Biology, and remained there for eight and a half years. During that time there was steady growth in the Department of Entomology at Cornell, and improvement in its equipment. There were four members of the teaching staff. William A. Riley had come on a fellowship in

1898 and had been made an instructor in 1901. A new course had been established in insect morphology, and in that course he had a large share. The Department was just leaving its outgrown quarters in White Hall for new and larger ones in Roberts Hall on the adjoining Agricultural College quadrangle.

In 1907 I returned to join the staff of the Department as Assistant Professor of Limnology. It was given to me to initiate a new course of university instruction, and to break ground in a little-developed field. I count myself as very fortunate in having had Professor Comstock for my sponsor. He prepared the way for me. He persuaded Jared T. Newman, an honored and farsighted trustee, to give the University land at the head of Cayuga Lake for a biological field station. Liberty Hyde Bailey, then Dean of the College of Agriculture, provided a station building. Delavan Smith, of Lake Forest, Ill., a public-spirited friend, provided initial equipment and support. The need of both teaching and research in the undeveloped resources of our inland waters was recognized without argument by these men.

My own special field of limnological research was to be the biology of fresh water insects, and that justified my placement in a department of entomology. The wet land and its open waters, being a part of the land in whose animal population aquatic insects play a very large role, made desirable the alignment with a college of agriculture and an agricultural experiment station.

On the northwest corner of the second floor in Roberts Hall there was an excellent classroom that I was assigned for work in limnology. My first class numbered six students, four of whom later became my colleagues on the Cornell faculty: Hugh Daniel Reed, Albert Hazen Wright, Arthur Augustus Allen, and John Thomas Lloyd. Lloyd became my assistant. Later, as Instructor in Limnology, he joined me in authorship

of a textbook for the course, *The Life of Inland Waters* (1916). Professor Comstock gave our work in limnology whole-hearted support.

His department was run with economy and true efficiency. In all his relations with his helpers, from janitors to assistant professors, Comstock's method was to assign the work to be done, arrange fit conditions for doing it, and then keep out of the way. He didn't ask for reports at stated intervals; he asked only for reasonable accomplishment.

He was a bit short-tempered with any who shirked. Dr. Robert Matheson relates that one morning he entered White Hall with Professor Comstock, who, on going into his office, found his wastebasket full of paper scraps left over from the work of the day before. The janitor had neglected to empty it. Comstock seized the basket, carried it out into the hall and dumped the contents down the stair well, scattering them all the way down to the basement. Then he remarked casually that he had reminded the janitor often enough—perhaps he would now remember the wastebaskets.

Retirement from teaching service meant for Professor Comstock a golden opportunity to put the results of his lifelong studies into final form. He kept his office in Roberts Hall, and went in and out daily, setting a good example of productive scholarship to all the Department. First he completed a book that was a summary of work in his own special research field, *The Wings of Insects* (1918). A leading British entomologist said that Comstock's work in this field was the greatest contribution to knowledge of entomology in half a century. Then he put together another larger book that embodied the subject matter of his basic course, *An Introduction to Entomology* published in 1920.

IT HAS been said that many an institution is "the lengthened shadow of a man": the Department of Entomology

at Cornell University is the lengthened shadow of a man and his wife. During the first half-century of the University they were colaborers in the best sense of the word. Their place in its history is secure.

They were a complemental pair. He was short in stature, quick-spoken, alert, even fidgety sometimes, and always masculine. She was tall (fully his equal in height), slow-spoken, tactful, and gracious. They were alike in their aims and interests, in their spirit of helpfulness toward students, in their loyalty to the University and to every good cause that needed their support. Though they had no children of their own, their house became a second home to children of others, and a place of happy social intercourse to hundreds of students. Their lives were devoted to sound learning and sane living.

All around the world today there are many for whom the choicest memories of their college years are the evenings spent in the home of the Comstocks. Mrs. Comstock would read to them choice bits of poetry and prose, while her husband, after seeing that all were welcomed and made comfortable, would sit as a listener among them. I sat with them often. It seemed to me that Mrs. Comstock's favorites were the Quaker poet Whittier, the naturalist Thoreau, and the storyteller Kipling. But she loved all good literature and her readings ranged from Emerson's philosophical essays to "funnies." Of the latter class, many will remember this one:

A little pig with a querly tail,
All soft as satin and pinky pale,
Is a very different thing by far
From the lumps of iniquity big pigs are.

After the publication of the *Manual and How to Know the Butterflies*, their names ceased to appear as joint authors of books, but their joint interest and mutual help went into every book that either of them wrote. He was as proud of her achievements as she was of his.

At the time of Professor Comstock's retirement from active teaching in 1914, Dr. David Starr Jordan wrote:

His marriage intensified his influence in every way. His home became the center of nature study, as of human friendliness. Scores of youth of promise at Cornell have owed as much to the personal sympathy of the Comstocks as to anything anybody taught them in school. Not one of them—men or women—but renders grateful tribute today, not to Comstock alone, but equally to the gifted and bighearted colleague, who, as helpmeet, has kept full step with him through all these years.

They had many distinguished guests. Some were foreign scholars, visitors to the University, whose activities were in different fields. All were simply and delightfully entertained. Mrs. Comstock told me that they once had a British entomologist for a week-long guest, at a time when they were without hired help, and she was doing her own housework, both cooking and serving. The guest, wholly self-centered and oblivious to household affairs, put his shoes in the hall outside his chamber door each night to have them shined. Professor Comstock, knowing the English custom, rather than have a guest disappointed, took the shoes each night and shined them himself.

Of their home life, I will let the two friends who know them best speak:

George Lincoln Burr said: "Their tastes were congenial. . . . Their home, in which I lived for years, and which I knew well from our college days, was one of the loveliest I have ever known."

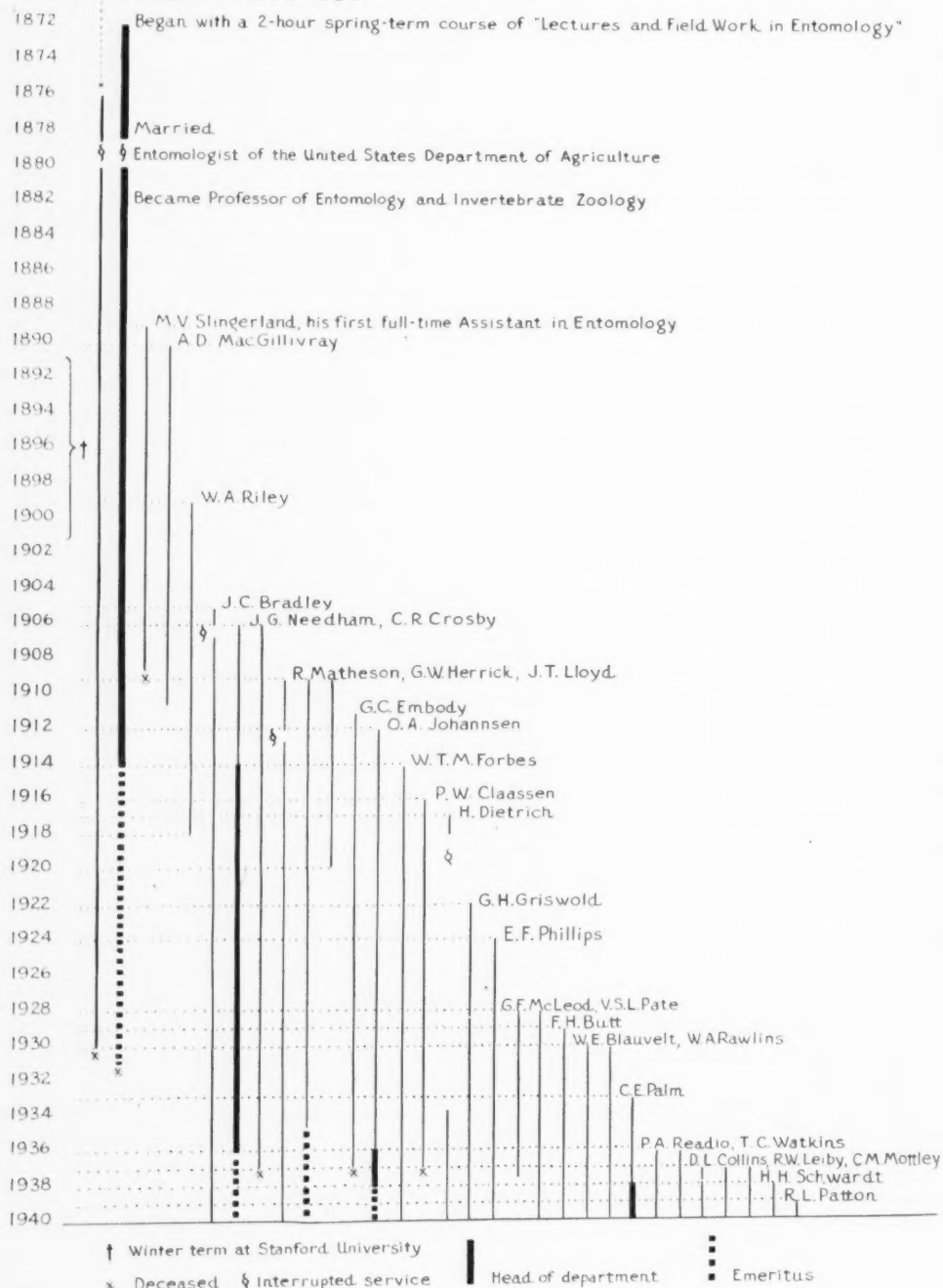
Simon Henry Gage said: "There was ever present in that home the glowing hearth-fire of human kindness."

Now, a bit more departmental history. In order to show, graphically and chronologically, the development of the teaching staff of the Department, I have prepared the accompanying tabular statement. My faculty colleague, Professor Bristow Adams, who was a friend of the Comstocks first at Stanford University and then at Cornell, has wrought

The Lengthened Shadow of a Man and His Wife

John Henry Comstock. 1849-1931

Anna Botsford. 1854-1930



it into acceptable form for presentation. Perhaps I may be allowed to call it a *shadowgraph* because of its special function: it slants like a shadow from the two great founders. It will serve as a condensed record for the first 68 years of the University, with names and dates of entrance of the principal members of the staff of the Department down to the year 1940.

It shows at the top the long initial period of struggle for recognition, while as yet entomology had no place in university curricula. It shows that for nearly half of the years of his teaching service Comstock was without a single paid assistant. These were years of slow and steady progress. After them came a period of rapid expansion of the Department, with belated recognition of the service that Comstock was rendering. The public was beginning to learn the importance of insects in agriculture and in public health. No one of intelligence now scoffs at the study of insects. Greater financial support was coming to the agricultural colleges and their associated experiment stations. The work in nature study had turned the eyes of the children toward Cornell University, and they were flocking to the College of Agriculture in rapidly increasing numbers.

The shadowgraph does not show two shifts of boundaries that were made during the latter years of the Comstocks, when I was head of the Department. One was the shift of nature study into the Department of Rural Education. This change was made in Mrs. Comstock's time and with her approval.

The other transfer was due to change of sources of financial support. It involved vertebrate zoology. It was a double shift, first, into the Department of Entomology, and, some years later, out again; the work in ornithology under Dr. Arthur A. Allen, and that in systematic vertebrate zoology under Dr.

Albert H. Wright. These two are omitted from the shadowgraph because their work was in no part entomological. As head of the Department when they were in it, I want to say that I was very proud of the work done by these two colleagues and their helpers, both in teaching and in research. The temporary association was a happy one.

There is no need that I should write of the work of my fellow-teachers who have come into the Department in my own time. That is recent history. With new men have come new courses of instruction: medical entomology, aquiculture, insect ecology, insect embryology, etc. Increase of knowledge has brought specialization. The work in applied entomology has been greatly expanded, its tasks differentiated. There is not space to speak of the work in extension, or of the great changes in the field of economic entomology; of the new methods and shifts of emphasis accompanying better knowledge of insect physiology and new discoveries in insecticides.

Among the things least changed by time is Comstock's basic course in general entomology, which was taken over first by Professor G. W. Herrick, and, after his retirement, by Dr. Robert Matheson. Most worthy of preservation in the Department is the Comstock tradition of sincerity, reverence for truth, clean living, good fellowship, and human kindness, of work for the joy of the working, and for the spread of the resulting public benefits.

So at the end of this, my partly recorded and partly remembered tale, it comes about that the story of the development of the first university Department of Entomology is also in brief the story of the life and times and teamwork of John Henry and Anna Botsford Comstock, the like of which we shall not see again.

NATURALISTS FOR THE FOREIGN SERVICE

By KARL PATTERSON SCHMIDT

CHICAGO NATURAL HISTORY MUSEUM

THE TERM naturalist covers a somewhat vague category of persons interested in the natural sciences, either as amateurs or professionals, but in either case with the implication of a breadth of outlook that removes them from the more precisely definable botanists, zoologists, and geologists. My own definition of a naturalist as being a biologist who has traveled will not bear much inspection. It may be agreed, however, that naturalists are those persons who take a keen delight in their natural surroundings, and that at best they may combine the objectivity of the scientist with the subjective warmth of the artist. Difficult though they be to define, naturalists are well-known among us, and seem to have some capacity for self-preservation and for the propagation of their kind. Naturalists are not always appreciated by the general public, though they make the best of teachers, and, when their natural history is an avocation, may include representatives of such respected professions as banking, the law, "the cloth," or engineering.

The fundamental generic quality of a capacity for active interest in a particular environment, has suggested to me a distinctive use for naturalists. From intimate and long-continued personal observation of the United States Foreign Service, I envisage the possibility of bringing together in the public interest an important demand and need for personnel and an unrecognized source of supply. Realizing that the proposal is by no means a new one, I propose the wide use of naturalists in the field of foreign relations, and specifically in a much-needed expansion of the American foreign service at its several levels.

The inadequacy of our prewar foreign relations personnel has been pointedly established by the experiences of the second World War. In Peru I once made a thousand-mile journey to reach the nearest consulate, and in 1929 there was no official representative of the United States between the Fiji Islands and Java. In non-European and in many small countries we have for the most part been represented only at the capital city.

It therefore requires no great perspicacity to predict that the foreign representation of the United States, in diplomatic and consular posts, will require very great expansion in the postwar era now facing us with its problems. It is commonplace to predict greatly increased and world-wide air travel. On the ground, the highway from Alaska to Tierra del Fuego is approaching completion. Even New Guinea, the last great reservoir of the unknown, has suddenly become accessible. The extent of the expansion required may at present only be guessed at. At a venture, from a glance at the map, I should recommend a fivefold increase in the number of our foreign posts and a tenfold increase in personnel as a minimum, if we are to establish some real contact with other nations and other peoples.

If there is to be any expansion at all, an immediate problem arises, for the State Department is already faced with difficulties in staffing its more remote posts; and new consulates must be established in still more remote regions. At best the average American citizen does not take kindly to foreign service, and is likely to find himself bored, finally beyond the limits of his endurance, in a

tropical locality, or in any locality lacking his standards of civilization.

There are good reasons enough for disliking many a tropical situation. Monotonous heat and glaring sunshine, too frequent rains, continued cloudy weather, or too much wind may set the stage with a climate intrinsically difficult for persons adjusted to the Temperate Zone. Insect pests in unfamiliar variety and in pervasive hords may be present. The disagreeable features of a human environment often dirty, not without danger from unfamiliar diseases, and usually correspondingly and exasperatingly inefficient, added to an unfavorable climate, may readily come to dominate one's whole attitude unless there is a counterbalancing interest in something in the country itself, something that is to be found nowhere else.

Every kind of foreign service would profit if we had the means of filling the more out-of-the-way stations with persons who, far from being bored in them, would regard the opportunity to live in such places as a privilege. On the average, at least, naturalists as a class present precisely this characteristic. Boredom is so impossible to the naturalist that it seems a cardinal sin. Everything different from his environment at home offers him something to study. The very weather is a natural interest. An attempt to understand the physiography of his area may lead him on one hand into geological studies, and on the other into the inexhaustible problems of the distribution of plants and animals. We think of our world as geographically explored; but its exploration for the smaller animals, for the limits of distribution of both animals and plants, and especially for the understanding of those limits, is only just getting under way. To a naturalist it seems evident that the employment of trained or self-trained naturalists in all faraway consular positions would thus contribute to the solu-

tion of a variety of scientific problems, and would at the same time solve the fundamental personnel problem of foreign service.

At a fairly simple level, our naturalist in a foreign post need be no more than a collector, learning to collect effectively as he corresponds with the museums or with the specialists to whom he sends his collections. Collecting plants and animals may, of course, become one of the most exciting of occupations, involving all of the elements of hunting for sport, with far more significant and permanent rewards. It is grand and romantic to search for the fossil remains of the animals of past ages, or to collect a series of beetles that prove to represent a "new species," or to contribute specimens valuable in the attack on important biological problems. A dozen museums would be delighted to correspond with such amateur collectors at tropical stations, and amateur collectors frequently develop into first-rate naturalists.

My suggestion really aims much higher. We should send trained scientists to *become* naturalists in these foreign stations. Young people already trained in one or more of the biological, geological, geographical, or anthropological sciences, and already filled with the determination to devote their lives to these sciences, are available in the graduate departments of almost every university in the country. There is no reasonable doubt that young men and women of this type could fulfill the normal duties of a consular office with distinction. A university group especially suitable for the foreign service is presented by the graduate students of geography. Within my own generation, the appreciation of human geography as an adult interest has led to the establishment of a flourishing department of geography at nearly every large university.

There is a considerable tradition for the pursuit of scientific interests in con-

junction with foreign service. Now, on the threshold of an age in which our lives are to be dominated by science as never before, such association gains vastly in importance. It may, indeed, be brought into relation with any program for the fostering of scientific research under government auspices.

My own experience points to a factor of vital importance to the success of our foreign representatives. Anyone who positively dislikes the foreign environment in which he is placed cannot well establish friendly relations with the people among whom he finds himself, and he is not likely to make the effort even to learn their language. With respect to the language, the naturalist is vitally concerned with learning it as a tool for the pursuit of his scientific studies. It has been my own familiar experience in Latin America to find that however bad one's Spanish, the attempt to speak it is seized upon as evidence of sympathy and of interest in the country itself. The naturalist who expresses specific curiosity about *anything* native to the country in which he travels finds his interest regarded by the residents as a profound compliment. His activities, in fact, afford a continuing avenue of contact with the foreign society in which he is located, extending from the urchins who bring him specimens at a centavo each to the most cultured persons encountered. I hesitate to affirm, but I have often suspected, that naturalists bear a better reputation abroad than at home in our United States.

To make my proposal concrete, it may

be pointed out that the salary of a foreign service employee begins at \$2,500 per annum, with certain other allowances at the more distant stations, and with transportation for himself and family. After a minimum stay of two years, his return expenses, including those of his family, are allowed. Training in specifically foreign service duties is given in a special training period, with pay. Let us suggest a three-year contract. At its termination the State Department could offer further opportunities to a staff member with considerable experience and good educational background. If the young man should decide against further foreign service, he can return to his graduate work in a university with a rich addition to his real education, and in many cases with the material for a thesis of genuine value. Above all, in the teaching career to which he may turn for his further livelihood, his foreign experience will command prestige and give him an invaluable contact with his students.

I believe that a cooperative arrangement between the graduate schools of the universities of the country and the State Department, for the supply of staff to foreign posts, would redound to the credit of both, and prove profoundly beneficial to the nation. Such an arrangement might be coordinated by the National Research Council. The possibilities of benefit both to the foreign service and to science and scientific education should receive consideration in government plans for aid to the scientific activities of the nation.

THE SCORING OF ATHLETIC CONTESTS

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IN ATHLETIC contests, the word "team" is used in two different senses. In the first sense, team is a group of associates who subordinate personal prominence to the efficiency of the whole; a football team is an example. In the second sense—and a track team is an example—team is a group of individuals, each of whom has his specialty, which he performs to the best of his ability, with essentially no help from his teammates.

For teams in the first sense, either comparative scores or absolute scores are practically meaningless. Varying excellencies in different departments of play make it a common thing for team A to beat team B, B to beat C, and C to beat A. Further, the subordination of personal prominence must be so complete that an individual must, for the sake of his team, be ready to perform in a manner inferior to the best he can do. Examples are the intentional base on balls in baseball, and the intentional safety in football. We conclude that in football, baseball, lacrosse, hockey, basketball, soccer, and polo the scoring systems involve little that is worthy of scientific study. And we note explicitly two paradoxes:

The Ring-Paradox. This consists in A beating B, B beating C, and C beating A.

The Control-Paradox. This consists in an inferior performance by which defeat is turned into victory.

As examples of teams in the second sense, we have track, cross-country, swimming, boxing, tennis, golf, gymnastics, fencing, wrestling, rifle, winter sports, and the like. In many of the events which occur in these sports, the whole idea is to get somewhere first, or to throw something farthest. The situation

is more complicated in boxing, wrestling, tennis, the fancy dive, fencing, and form in ski jumping, but that need not concern us here. It is true that in running a race one may be bothered by his opponents, or helped by a teammate, but this influence is on the whole small, and in a study of scoring systems may be disregarded. Further, to simplify the situation, we omit entirely the human element. In this discussion, our runners must always run a race in the same time; our jumpers must always jump the same distance. All contestants are reduced to automata who always give their best performance, and this best performance is a constant, unless—and this will probably seem unlikely to the reader—the control-paradox occurs in the scoring systems, and our contestants take advantage of it.

Now when representatives of two teams run a mile race, it is easy to determine the winner. He is the man who finishes first. In a javelin throw, it is the man who throws the javelin the farthest. But when we come to combine a mile race with a javelin throw, we introduce complications. Shall we merely score the winners (as the British do)? Or shall we count seconds and thirds (as is common in the United States), and, if so, how much? Shall a superlative throw of the javelin count a little more than a narrow victory in a rather mediocre mile?

We must have definitions. Quite generally we may say:

A *team* consists of individuals, whose individual performances contribute to a common score, with the purpose of establishing the general superiority of their team over one or more other teams.

An *event* is an athletic contest in which one or more individuals from two or more teams participate.

A *meet* is a group of events. Its purpose is to determine the order of general excellence of the teams involved.

A *scoring system* is the technical device by which this order of general excellence is established. It is assumed to be impartial, and adapted to the peculiarities of the events which it measures. The independent variables of a scoring system are the times, distances, orders, etc., of the individuals in an event. Using these variables directly, or others dependent on them, the system assigns *event-numbers* to each team in each event. It is assumed that the event-numbers monotonically increase or decrease with team excellence in that event. It is assumed that if "increase" is the pattern in one event, it is the pattern in all events. It is finally assumed that the event-numbers for a team are merely added together to give a *meet-number*. Order of excellence is established by the order of the meet-numbers.

The essence of a scoring system is the method of assignment of the event-numbers. These methods may differ radically in the orders of excellence which they establish. Suppose 2 golfers A and B play 3 holes, and suppose their scores are:

A	7	3	3
B	4	4	4

In the scoring system called medal play, these numbers are themselves the event-numbers, and B beats A 12 to 13. In the scoring system called match play, the significant thing is winning the event, and for this the event-number unity is given; hence the event-numbers are:

A	0	1	1
B	1	0	0

and A beats B by a score of 2 to 1 (technically one-up). On the evidence furnished, B is better than A at medal play;

A is better than B at match play; and the question as to who is the better golfer, being undefined, is still unanswered.

The reader will note a close analogy between this and the election of a President of the United States. The Electoral College (match play) yields a result which may be quite different from the popular vote (medal play).

But note that match play involves the ring-paradox, whereas medal play does not. A single example suffices to show this. Suppose 3 golfers play 3 holes—remember they are automatons—and A has scores of 7, 3, and 4; B scores of 2, 4, and 6; and C scores of 3, 5, and 2. Then, in dual match play we have these results (the winning score is italicized):

A	7	<i>3</i>	4	B	2	4	6	A	7	<i>3</i>	4
B	2	4	6	C	3	5	<i>2</i>	C	3	5	<i>2</i>

Thus match play can produce the ring-paradox; and obviously medal play never can. But, in my judgment, this is no valid reason for saying that therefore medal play should be preferred.

The ring-paradox is, in fact, inherent in many more systems than is generally supposed. Consider a cross-country triple meet (actually a meet of 1 event) in which 5 representatives of each of 3 teams compete. The only thing that matters is the order of the finish. The winner is assigned the number 1, the next man the number 2, and so on. The numbers of the individuals on a team are added for a team score, and low team score wins. Suppose the order of the finish is:

A	B	C
1	6	2
4	7	3
5	8	11
14	9	12
15	10	13
39	40	41

We can now say that in a triple meet, team A is best. But it by no means

follows that we can draw any conclusions as to what will happen in dual meets. A glance at the numbers shows that in a dual meet between A and B, A will take the first 3 places, B the next 5, and A ninth and tenth. The results of 3 dual meets are:

A	B	B	C	A	C
1	4	3	1	1	2
2	5	4	2	4	3
3	6	5	8	5	6
9	7	6	9	9	7
10	8	7	10	10	8
25	30	25	30	29	26

an excellent example of the ring-paradox.

The question of which scoring systems involve the ring-paradox and which do not can be completely answered after an analysis of the following ring-paradox, the simplest that exists. Three teams consist of a single individual each, and they compete in 3 events. In event (1) A always wins, then B, then C. In event (2) the order is B, C, A; in event (3) the order is C, A, B. Now consider these 3 dual meets, in which winner scores 1 point in an event:

	A	B	B	C	A	C
(1)	1	0	1	0	1	0
(2)	0	1	1	0	0	1
(3)	1	0	0	1	0	1
	2	1	2	1	1	2

This gives us the clue. In (1), A is one better than B, B is one better than C; yet A is only one better than C. Somehow, the scoring system should make A *two* better than C. More generally, it can be demonstrated very easily that if the symbol $S(A, B)$ means the amount by which A's event-number exceeds B's event-number, the necessary and sufficient condition that a scoring system avoid the ring-paradox is that the following functional equation hold:

$$S(A, B) + S(B, C) = S(A, C)$$

The reader familiar with scoring rules

can now verify that the ring-paradox is inherent in the following: track, cross-country, swimming, gymnastics (American rules), winter sports, boxing, tennis, fencing, wrestling, and match play at golf. The ring-paradox can never occur in rifle, bowling, gymnastics (Canadian rules), medal play at golf, the pentathlon, and decathlon.

Can scoring systems be devised which would eliminate the ring-paradox? It is easily demonstrable that to do this the system must produce an *absolute* rating $S(A)$ for each team in each event: that is, $S(A, B) = S(A) - S(B)$. An absolute rating in boxing, tennis, fencing, or wrestling is absurd. An absolute rating in golf simply means that you discard match play for medal play; and in gymnastics it means that you adopt Canadian rules. In track, cross-country, swimming, and winter sports, absolute ratings are possible, but in my judgment are highly undesirable. In a track meet it would require the determination of the time of every runner by electrical devices at least to the nearest .01 second. It would mean an appalling amount of mathematical computation. The winner of a close race would not be sufficiently rewarded. And most unreasonable and intolerable is the fact that team B might win 14 first places and 15 second places in 15 events, and yet be beaten by the superlative effort of one individual on team A, performing in only 1 event. The ring-paradox is much easier to endure than these absurdities.

The control-paradox is not as common as the ring-paradox. Its actual occurrence in the sports listed requires (a) two mutually antagonistic systems unwisely interlaced, or (b) a single illogical system. Several examples of (a) are known to exist; they are more amusing than harmful. I know of only one example of (b), a method used in scoring the *slalom* in skiing.

As an example of control-paradox,

variety (a), consider a "meet" which consists of a triple track meet with its 15 events in which first, second, and third count as usual, 5, 3, and 1; and in addition a sixteenth event, a cross-country run, in which the winning team is determined in the usual way, and then 5, 3, and 1 points are arbitrarily assigned as event-numbers. The cross-country race is held last. Prior to its running, the scores are:

A	B	C
48	45	42

In the cross-country race, the sixteenth event, the order of finish is:

A	B	C
9	2	1
12	5	3
13	6	4
14	7	10
15	8	11
63	28	29

Whereupon team B is awarded 5 points; C, 3; and A, 1. These added to the totals above give B, 50; A, 49; and C, 45.

But suppose the A-man who finished ninth, had sat down on the grass, and waited for the two C-men, whom he could have beaten, to finish ahead of him; and then casually strolled across the finish line. We should then have:

A	B	C
11	2	1
12	5	3
13	6	4
14	7	9
15	8	10
65	28	27

By this maneuver, team C, which was not the dangerous competitor, is handed the victory, and gets 5 points, B gets 3, and A is no worse off with 1. The final score is:

A	B	C
49	48	47

Thus by performing as badly as is humanly possible, team A was able to turn defeat into victory. The ability to "control" the scores of adversaries is widely prevalent in triple meets, but in general a team which exercises this control loses more than it gains. This example is an unusual exception.

A scoring system, widely used in winter sports in the *slalom*, downhill, or *Langlauf*, inherently contains the control-paradox, variety (b), even in a dual meet. In this system, the winner (the man with the least time) is given 100 points. Every other competitor is given a fraction of 100 points, the numerator of the fraction being the winner's time, and the denominator, his own time. Now consider this situation for two 4-men teams:

Team A: Time	Points	Team B: Time	Points
79	100.00	80	98.75
81	97.53	130	60.77
82	96.34	131	60.31
83	95.18	132	59.85
	389.05		279.68

The totals (or, in practice, one-quarter of the totals, which does not affect the situation) are defined as the event-numbers, and A is ahead by 109.37 points. But suppose the winning A-man had slowed down to 80 seconds. The score would then be:

Team A: Time	Points	Team B: Time	Points
80	100.00	80	100.00
81	98.77	130	61.54
82	97.56	131	61.07
83	96.39	132	60.61
	392.72		283.22

Team A is now ahead by 109.50 points. Thus if the winning A-man is willing to give up his personal victory, and accept a tie for first place, he can thereby strengthen his team's score, and in a close meet turn defeat into victory.

But although the paradox exists whenever one team is sufficiently superior to the other, it does not seem to be of much practical importance. In fact, when a winter sports meet involves 10 or 12 teams, the extraordinary amount of calculation involved in this method seems (at least to one who has often assisted in scoring) a more valid objection than the hazard of the control-paradox.

In conclusion, in judging any scoring system, or in formulating any new one, the first question to ask is: How many men do you want to have contribute scores in any event? Meets in boxing, tennis, fencing, wrestling, and golf usually consist of several 2-man events, and the winner alone contributes to the score; 1 point for the winner and 0 for the loser seems to be about as good a solution as any. In track, the British answer is 1, the winner. The American answer is 3 (or even 5 in a multiple-meet), and the desire for "balanced" teams has led to an increase in the value of seconds and thirds. Formerly, the system 5-2-1 was used; now it is generally 5-3-1, and in multiple-meets, 5-4-3-2-1. In swimming and gymnastics, 3 places count in this country. There seems to be no valid reason for changing any of these systems.

In cross-country, the answer is different, as 5 men on each team are to contribute to the score. The emphasis here is on team balance. In a multiple-meet of 15 or 20 teams, it is not unreasonable for no member of the winning team to finish among the first 10. Again, it is perfectly possible for a team to have 4 men finish first, second, third, and fourth, and yet lose the meet through the indifferent performance of the fifth member. These facts are well-recognized, and are not considered undesirable. Further, a

cross-country meet is unique in that it is a meet with only one event, and therefore it is reasonable to keep the scoring system which is now used.

American winter sports meets constitute the real exception. In each event, 4 men on each team are to contribute to the score. No other sport has ever put such emphasis on team and balance, and, in consequence, no other sport has ever faced such difficulties in formulating a scoring system. There is no need for reviewing all the systems which have been tried. They are all complicated, as indeed they always must be. A good scoring system should be capable of easy explanation to the judges, contestants, newspaper reporters, and general public. It is desirable to have a system which can be kept up to date as a meet progresses, and which will yield the final results soon after the end of the meet. But when 3 competent scorers, all professional scientists, equipped with slide rules and electric computing machines, require 4 to 6 hours to determine the winner, I would venture the categorical statement that the scoring system is a bad one. Further, if the performances of 4 men on each team are to be considered significant, I know of no system which could prevent this situation: team A finishes first, second, and third in every event, and also finishes fourth in every event but one, and yet loses the meet. These difficulties are very real, and they greatly outweigh the minor abnormalities of the ring-paradox and the control-paradox. The proponents of winter sports believe 4-man teams in each event are desirable, and they have good arguments for their position. But they must always pay a high price in the complex and undesirable features of any possible scoring system.

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SOCIAL PARASITES AMONG BIRDS

By ALDEN H. MILLER

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THE WAYS of a parasite are opprobrious to men, who subconsciously judge them by their own code of ethics. We are pervaded with a sense of fair play and by democratic ideas of equality of opportunity to such a degree that the animal parasite, as indeed the predator, tends to assume a sinister aspect. No technical biologist will admit anthropomorphic bias in interpreting the actions of animals; but, as rigidly mechanistic as he may be in his explanation of their behavior, we note his interest is often piqued by a situation which by human standards is irregular or insidious.

In the unmoral animal world, success rather than virtue is the keynote, success in maintaining life and in reproducing. Any means to these ends is biologically acceptable if it works. Parasites display remarkably neat methods of attaining their objectives, methods quite as elaborate as those of free-living animals. But a parasite makes sacrifices in becoming dependent on another species. It narrows its own evolutionary possibilities and limits its expansion in numbers. It casts its fate with its host and it cannot overuse its "meal ticket." The host's welfare as a species is of especial importance to the parasite, and it is usually regarded as axiomatic that the most successful parasites are those that do not cause the early death of their hosts.

Parasitism as an animal industry has offered many openings—opportunities for developing the art in diverse ways. Like any unoccupied ecologic niche or industry, it becomes filled by progressive modification of pre-existing free-living animals. Similar kinds of parasitism

have evolved independently a number of times, each arriving at a similar end state by different routes and from different starting points. Such has been true of social parasitism among birds.

Social parasitism is in some respects a poor term for what I describe, but it is now well-accepted English usage. It refers to the parasitism of the nest of one species by birds of another species, and the consequent dependence on the host to incubate the parasite's eggs, and to raise and protect its young. Strictly speaking, this is not parasitism of the society of which an individual is a member because it is not an intraspecific affair. Only in the sense that many bird species are conceived as forming one ecologic society is the term justifiable. The German term *Brutparasitismus* is, on the other hand, entirely accurate.

The fact that the European cuckoo is parasitic was known in Aristotle's day, but lack of precise information about its activities prevailed until two decades ago, in spite of the great fame this bird gained from its unusual reproductive methods. Misconceptions are evident in the meaning of the word "cuckold," based on the cuckoo's supposed habits. The implication in this word is that the female cuckoo is unfaithful to her mate. There is not much good evidence to show that she is; she merely passes off on someone else her domestic duties of raising young, while she spends her time in the job of finding the nests of victims. Some modern society women do much the same thing with their own children. In fact, the term social parasite might more correctly be applied to them, for such women are parasitizing

their own society, their own species—but this observation may have thermomorphic bias.

Not until the early nineteenth century was it discovered that some birds other than cuckoos are parasitic, and only in the past few decades have several more groups been added to this category. We now know of parasitism in five separate families of birds, belonging to four distinct and not closely related orders. Obviously it is an independent development in each group, stemming from ancestors that had normal nesting and brooding habits. The groups are: the cuckoos of the Old World (not the North American cuckoos); the honey guides of Africa; the cowbirds of North and South America; the weaver finches of Africa; and the ducks (the Argentine black-headed duck only).

To understand the origin of parasitism we must review a few elements of normal breeding behavior.

(1) In a great many birds, as the nesting season approaches, interest is taken in a particular area in which the nest later will be built. This area, or focal points in it, is defended, especially by the male, against others of the same species. It is called specifically the "territory" and it serves variously, according to the species, one or all of the following functions: protects the mate and isolates the pair, thus preserving the bond between the members of the pair and in general promoting monogamy; protects the nest from molestation; assures a food supply—an adequate source area for food—for the raising of young.

(2) Either before or after establishment of territory by the male, pair formation takes place in monogamous species. Nuptial display, common interest in a territory, courtship feeding, and nest-building may serve to strengthen this bond, which usually is held in force for the period in which a brood of young is being raised. The sexes seem to be

recognized initially either by characteristic marking or by behavior, but recognition of the mate soon becomes a matter of individual recognition, of which birds are fully capable.

(3) Nest construction takes place after pairing, usually long after territory is established, although in some species the establishment of territory may follow upon selection of the nest site and consist of an expansion of the sphere of protection centering at the site. Nest construction may be engaged in by both male and female or by the female alone. It stimulates sexual activity, and parts of the building activity may be taken over as an element of courtship display, thus at times contributing not at all to construction of the nest. The architecture of the nest is usually characteristic for the species. In its broad aspects, and even in many details, it is hereditarily determined, as are most actions of birds.

(4) Ovulation, or laying, is not purely a culmination of a seasonal reproductive cycle set in motion by the endocrine system, in timing with daylight or other external cycles, but is further dependent in most species on the stimulus of courtship, nest construction, sight of and feel of the nest, and immediate conditions of food and weather.

(5) Once the eggs are laid, the chain of subsequent activity, of brooding, of feeding young, and of protecting them, follows, governed by internal events in the endocrine system and by later external stimuli, such as the hatching of eggs and the begging of young. The male responds to these situations as a result of external stimuli; yet in some species his activity in brooding and in raising the young may be even more intense than that of the female.

By its brevity the foregoing sketch does violence to many special fields of behavior study, and would require much elaboration and some qualification for

any particular species. However, it may be thought of as applying fairly well to most small songbirds and to the ancestral types in several groups that have evolved parasitism.

Steps in the modification of instincts in the cowbirds fortunately can be traced in part through two South American species (Friedmann, *The Cowbirds*, 1929), one of which verges on parasitism yet still raises its own young. The other has an imperfect parasitic procedure, probably but recently evolved. The first of these is the bay-winged cowbird (*Molothrus badius*), of Argentina. It pairs up in the spring and is strictly monogamous, each pair sorting out of the winter flocks and going its own way. But instead of establishing territories before, or even following, pairing the pair wanders about looking for old or empty nests of other species. Often they fight with the rightful owners and usually are successful in ousting them. They even throw out the eggs and young from occupied nests. Having thus taken possession of either an old or an occupied nest, they lay their own eggs, incubate them, and raise their young. After acquiring a nest, they establish a territory radially around it and proceed to renovate the nest, sometimes adding significantly to it. Indeed occasionally they build their own nests and do quite well. Thus they have not lost the instinct to construct. It should be pointed out that the family to which the cowbirds belong is noted for its elaborate, well-built nest structures (the New World orioles are members of this group).

The significant departure from the norm in this species seems to be an upset in timing, that is, an aroused interest in nest structure before a home territory is established. There is overconcern for nest structures per se and special susceptibility to the stimulus of the sight of a nest of whatever kind. Territorial instincts are not only late in manifestation

but are in general not strong. One further point to be noted is the weakness of the brooding and feeding instincts of the females; the male does the major share of the work at the nest.

The screaming cowbird (*Molothrus rufo-axillaris*), of the same geographic area, is closely related to the bay-wing, and, according to Friedmann, principal student of these birds, has certainly been evolved from it. Screaming cowbirds are monogamous, and may even remain in pairs throughout the year. They establish territories but defend them very weakly, often tolerate other pairs, and may shift about from one place to another during the early part of the spring season. Egg-laying is long delayed; and, when it does finally occur, the only nesting activity of other species still in progress is that of their relatives, the bay-wings. They then lay in the bay-wings' nests, parasitizing this species solely.

"Assuming that in most ways the original habits of the screaming cowbird were similar to those of the bay-wing, we would expect that the birds tried to [take over and] breed in nests of ovenbirds, woodhewers, etc. . . ., but tried to do so early in the season. . . . The struggle for nests is much greater [then] . . . than later on, and the screaming cowbird, handicapped hereditarily by a weakened territorial instinct, probably could not succeed in this struggle. . . . [The frequent desertion of territories by this species] indicates very strongly that the weakened territorial instinct of the male often is insufficient to maintain its influence long enough" for the egg-laying of the female to follow in normal sequence. With no adequate defense of a stolen nest, the female laid in nests the pair attempted to acquire. As in the ancestral bay-wing, the female was weak in her brooding instincts, and now in this species the protecting and brooding instincts of the male also became weak-

ened and the bond between pair and nest broken. The eggs were then left to the foster parents, who, never having been driven away effectively, returned and raised the parasite and perhaps some of their own young. The screaming cowbird was delayed until late in the spring because of the difficulty in getting at nests. At that time the nests most available were those of the bay-wing, who was itself none too aggressive in defense of its nest. The screaming cowbird thus could most readily succeed in parasitizing this species and is now found to do so exclusively.

In the North American cowbird (*Molothrus ater*), as in several South American species, parasitism is developed more perfectly. In these, territorialism has in effect disappeared. There is, to be sure, interest in or attachment to an area but no defense of it. When many cowbirds are present in a region, they are tolerant of one another. Also, they become promiscuous, or, more usually, polyandrous, for the males outnumber the females in these species. Further, they parasitize a wide range of species—kinds almost always somewhat smaller than themselves. There is no host specificity.

Thus, pair formation, territorialism, nest construction, brooding, and feeding instincts have all been lost. I think of a group of cowbirds that lived about a mesquite thicket where I camped one spring on the Tucson desert. A single female was the center of the show. Six males were in constant attendance, squeaking, and ruffling their feathers in courtship antics; they were not effectively aggressive toward each other. As the female flew from one tree to another, the flock of attendants moved with her in close formation; they seemed to be mobbing the female, as crows would an owl. For the female this looked, anthropomorphically, to be a carefree, though hectic, existence.

The devices of parasitism which in a sense replace the lost instincts are several. There seems no doubt that the female cowbird will be able to mate. The problems for the species now become: (1) finding nests in proper stage for parasitism; (2) insuring that the host is not disadvantageously disturbed by the deposit of the foreign egg; (3) insurance that the young will be fledged by the foster parents in competition with their own young. Several adaptations claimed to be effective in accomplishing these ends have with recent, more precise observation proved to be nonexistent. Only recently has the laying and egg-removing action of the female cowbird been exactly recorded.

The story runs as follows: The female takes a sharp interest in nest-building of other birds, and is stimulated by sight of this and perhaps by their courtship. On her beat, she finds numerous nests by observation of building activity and by special search in the plant cover. Not only are these found, but also they are visited and inspected. When all this interest induces ovulation, she has a supply of nests to use and is in touch with laying activity in them. She usually lays only after one egg of the host has been deposited.

Until eight years ago there had been reported only one satisfactory observation of a North American cowbird sitting on a nest, laying; cowbird eggs merely were known to appear in the nests of hosts. They were easily recognized by their characteristic markings. Patient work on the part of Hann (*Wilson Bulletin*, 1937, 1941) gave the clue to the difficulty. The cowbird lays exceptionally early in the morning before the host lays for the day in question. Normally, host birds do not stay on the nest at night until the clutch is nearly or quite complete. The cowbird, then, gets to the nest before the owner. Hann reports it was usually impossible for him

to make out the form of the cowbird when it was quiet, in the dim light. But his photoflash records tell the story. This early visit produces a minimum of disturbance at the nest. The egg can be laid in thirty seconds, and the hosts may be unaware of the unhappy event.

The practical difficulty of knowing in advance what nest the cowbird will visit can well be imagined. What nest shall the faithful ornithologist sit by in the dark before dawn? Some clues were found that aided materially. From study of the eggs in the nests of host species in a restricted area and with knowledge of the sequence of their appearance, it could be established that a female cowbird laid once a day, four or five days in succession; these groups of eggs, incidentally, correspond to the clutches of the same number laid by the nonparasitic bay-wing. If the observer has been as keen as the cowbird in finding nests, he will know which nests on the cowbird's beat will be in a favorable stage for parasitism. Further, Hann found that the cowbird usually made a close inspection of the nest of the intended victim on the afternoon of the day before. This action, observable in broad daylight, if one is at the right place, serves as a guiding sign, although it is not infallible.

Contrary to the belief held before Hann's photographic work, the cowbird does not remove an egg of the host from the nest when she visits to lay. Instead she takes it on the afternoon before, or later in the morning of her laying. This sneak visit, of course, can be very rapid and again will not cause prolonged disturbance of the hosts. About 85 percent of the time this removal takes place. Accidentally, the remaining eggs of the host may be damaged by the claws of the cowbird as she settles in the unfamiliar nest.

An egg is removed only if two or more eggs of some sort are present in the nest

when the visit is made. This instinct helps to insure that the cowbird will not remove her own egg; for, in an emergency, cowbirds may lay before any of the eggs of the host have been deposited. Only rarely does the cowbird make a mistake and remove her own egg or that of another cowbird that might have found the same nest. The eggs which are taken are speared with the bill and carried away to be eaten or crushed.

Host reaction is a subject about which much more information is needed. Once the disturbance of the cowbird's visit has passed, most hosts show little concern. We have some evidence that they are aware of the foreign egg, but seldom are they able to remove it; nor do they persist in efforts to do so, even though the egg may be twice the size of their own and strikingly different in pattern. Sometimes the host deserts the nest and starts afresh somewhere else. Certain species are much more readily disturbed than others, but qualitative data on desertion are sadly lacking. One reaction often seen in parasitized warblers is reconstruction of the nest, with a false bottom placed over the first set of eggs containing that of the cowbird. The warbler, of course, then lays another set.

Young cowbirds hatch with the host young, call lustily for food, and get it. Young of some species, such as orioles and warblers, may fail in competition; those of others, such as song sparrows, receive enough food to develop normally. Though larger than its host nestmates (sometimes 50 percent or more), the young cowbird develops rapidly enough to be ready to leave the nest with its associates. Like them, it is fed for ten days to two weeks subsequently and then is independent. Since most small birds feed their young on insects, young cowbirds are likely to receive an adequate, nutritious diet, regardless of the specific menu of the host. Certainly they are not fastidious.

Overenthusiasm in the search for adaptive devices and the influence of knowledge about cuckoos have led to some false ideas concerning the cowbird's special abilities in parasitism. It has been claimed that its eggs are unusually small, the better to resemble those of the hosts. In comparison with nonparasitic members of its family, the cowbird's eggs are no smaller; they average about 9 percent of the body weight. There is no increased thickness of the egg shell in adaptation to resisting breakage by the host during attempts at removal. The most persistent and favored notion has been that the cowbird's egg hatches before those of its host as a result of a shortening of the developmental period of the embryo. Again, the cowbird has speeded up the process not at all in comparison with its relatives, the American blackbirds. Its incubation period of eleven or twelve days is short, shorter than that of some of its hosts but identical with most of them. The cowbird's ancestral stock possessed certain characteristics helpful in parasitism, such as egg size and short incubation period. These properly are viewed as preadaptations. They seem not to have been enhanced in the slightest since the birds became parasites. Young cowbirds display no antagonism toward their nestmates. They make no efforts to pick at them, throw them out of the nest, or sit upon them. We shall see how this contrasts with certain cuckoos.

One study of the success of the North American cowbird has been pursued extensively enough to yield a reliable picture (Nice, *Trans. Linnaean Soc. of New York*, 1937). In the Middle West, song sparrows (*Melospiza melodia*) are favored hosts. Figures for survival in about 100 nests show 32 percent of the cowbird eggs laid were hatched and the young fledged. This compares with 36 percent, in general, for song sparrow

eggs in nonparasitized nests. The lesser figure for cowbirds may be attributed to occasional desertions by the song sparrows as a result of the appearance of the cowbird egg. Broods of song sparrows raised in nonparasitized nests averaged 3.4 young per nest; in parasitized nests, 2.4 young. Hence, each cowbird would seem to have been raised at the expense of one song sparrow.

The European cuckoo (*Cuculus canorus*) is more complicated in its devices for parasitism. Striking is its well-developed, though not always perfect, host specificity. Different tribes, or gentes, exist within the species, even in the same area, each adherent to a different host species and each specialized in at least one respect for that one species. Thus in England there are cuckoos parasitic on meadow pipits, and others that are tree-pipit cuckoos, hedge-sparrow cuckoos, or pied-wagtail cuckoos.

Much of the knowledge about these cuckoo tribes has been derived from close examination of the egg pattern. The situation is analogous to that in fingerprints. There is so much variability in spotting and coloration, but constancy in the product of one female, that individual identification often can be made, and the egg-laying history of particular birds can be satisfactorily followed if one persistently searches out the nests of all conceivable hosts in a restricted area. But, in spite of the remarkably wide range of individual variability of egg pattern, there are common elements in the pattern of each tribe, or gens; and these mimic in some considerable measure those of the host species of that gens. Often the mimicry is so exact that the cuckoo egg can be distinguished on first inspection from those of the host only by the texture of the egg surface or by the thickness of the shell.

Edgar Chance (*The Cuckoo's Secret*, 1922; *The Truth about the Cuckoo*, 1940) deserves credit for establishing

many critical facts about cuckoos. He followed the activities of a single female cuckoo for five seasons, finding her strictly limited to the meadow pipit (*Anthus pratensis*) as a host, except when he manipulated the supply of usable nests and was thus able rarely to force her to try other species.

Such a cuckoo maintains a definite territory, driving off other females; and she seems, as far as is known, to have one male in attendance—a principal spouse, if not an exclusive one. Maintenance of the egg mimicry of each tribe would require that the male be a member of the same tribe. We do not know how egg patterns in cuckoos are inherited, but it is highly probable that the pattern factors are transmitted both by male and female, not exclusively through the female in the Y chromosome peculiar to that sex. Under such conditions, the interest of the male in the same host as that concentrated upon by the female would be important. Definite pairing, within the gens, and territorial establishment in territories of the host species would tend to hold the gens intact and preserve the mimicry of the eggs in the tribe. Mimicry has been observed to break down in individual instances, we may suppose either because of cross-mating between gentes or through failure of a female to find a nest of her normal host when she is ready to lay. Presumably, the more frequent elimination of these misfit eggs by the hosts supplies the selective pressure that maintains the mimicry.

Territorialism in the cuckoo has yet another advantage. Only one cuckoo can be raised in a given nest, owing to the special reactions and requirements of the young. Hence, if two cuckoos lay in the same nest, one egg, or the young hatched from it, is sure to fail. In the economy of the species it is disadvantageous for females of the same gens thus wastefully to duplicate effort. Ter-

ritorial antagonism insures that this will not take place.

We may return for a moment to speculate on how it happens that young cuckoos return to parasitize the host species by which they were raised, for they must usually do this or the entity of the gens would be lost. Possibly cuckoos of different gens respond differently to the call notes, sight, and other actions of various potential hosts. That is, there may be some inherited recognition reaction peculiar to each gens. More likely, however, is the view that recognition of the host species is learned by each cuckoo in the long period when as a juvenile it is fed by that host. Attachments to animate objects other than their own parents, which are formed early in their lives, have been repeatedly observed in captive young birds of species with normal breeding behavior. It is to be expected that the migrant yearling cuckoo returning to its home range would evince an interest in the actions of its foster parental species, respond to its notes, and follow the course of its nuptial and nesting procedure. The response to its cuckoo mate, on the other hand, would have to be purely a matter of instinct—an inherited affair.

Mr. Chance devised a plan of study wherein he destroyed the nests of the host within the territory of the cuckoo on such a schedule that only one nest would be available in the proper condition when the cuckoo was ready to lay. Host specificity and territorialism made this feasible in the cuckoo, whereas it would not be so with North American cowbirds. By cultivating and managing the nest supply, Chance was able to be at the nests when cuckoos laid and to take critical motion pictures.

The following are the events in his story: A female European cuckoo lays every other day in the afternoon. The appropriate nests are found by watching and are visited in advance of laying,

much as in cowbirds. Before laying, the bird sits motionless on a lookout for an hour or two. This long period of waiting and attention to the intended nest is evidently the period when the egg is moving to the lower part of the oviduct; in fact, probably into the terminal chamber, the cloaca. She is then ready for a rapid and remarkable delivery. A few seconds before laying she glides down to the nest without wingbeats, in a peculiar, steady glide, suggesting that any undue exertion in the air might cause premature ejection. She alights on the nest and, because usually too large for it, crouches flat over it and drops or rolls the egg into the nest cup. In domed nests the cloacal area is merely pressed to the entrance, while the bird flutters and elings and the egg is projected inward.

Two attributes of the egg doubtless are important in this connection. First, it is small for the size of the cuckoo. It is but 3 percent of the body weight, compared with a normal of 9 percent. The laying of a small egg should be easier to control and to accomplish rapidly. Seven or eight seconds is the minimum time taken. Second, the egg is thick-shelled so that it can withstand rough treatment in deposition. Both these features are otherwise important in mimicry and in withstanding host attacks. The thick shell may have been an attribute that appeared incidental to reduction in size. The runt eggs occasionally laid by other birds are usually thick-shelled. The shell glands are equipped to deposit a certain amount of shell; if, then, a small ovum is presented, these glands seem to overload it with shell.

One of the most persistent myths about the European cuckoo is that it lays its egg on the ground and then carries it in the bill or within the mouth and thus places it in the host's nest. Several bits of circumstantial evidence have contributed to this untenable belief. A cuckoo

takes an egg from the host's nest as it arrives to lay, not before or after, as in the cowbird. While laying, it holds the host's egg in its bill and, following laying, it flies away conspicuously carrying the egg. The laying of its own egg is so rapid, and direct laying in domed nests seemed so unlikely, that lack of critical observation and the improbability of the true action maintained the erroneous belief. Chance's motion pictures of the behavior were decisive.

An extraordinary behaviorism, which has been repeatedly verified, is the action of the young cuckoo in ejecting its nestmates. Soon after hatching and while blind and largely naked, the young cuckoo thrusts itself beneath any object it comes in contact with in the nest, balances the object, young or egg, on its peculiarly flattened and depressed back, braces it with its stubby wings thrust backward and upward, and clambers with its load up the edge of the nest. At the rim it gives a sudden lurch and pitches its nestmate out. This instinct subsides after about four days, as found by testing older cuckoo nestlings.

Expulsion of nest competitors as a refinement in parasitism is doubtless especially necessary in a bird the size of the cuckoo. So large a bird could scarcely keep pace in development with the smaller host young. Also, if it is to grow adequately it must have the entire food ration which the small host species can deliver. Ludicrous indeed is the feeding of the giant cuckoo when it has attained its full growth, is out of the nest, but is still dependent. Small warblers which may be its foster parents have been seen sitting on its shoulders or clinging to its neck in order to reach its gaping mouth with the food. The hosts are occupied so long with raising the cuckoo that there is no time for subsequent nesting and, accordingly, they may raise none of their own young in that season. The parasite is here a

serious drain on the propagation of the host species.

The American cuckoos afford some clues to the origin of parasitism. In the nonparasitic members of the cuculine order, one notes instances of weakness in nestbuilding instincts—the nests are poorly built almost to the point of inadequacy. Irregularity in laying is another feature. Eggs are deposited at long and various intervals, as in the case of the California road-runner, where they may be laid in the nest after young from the first eggs have hatched and are half-grown. The timing of the series of reproductive instincts is thus poor. This leads to the occasional deposition of eggs in nests of other species of birds when their own nests have, through inadequacy, become destroyed, or when eggs are produced after their own nests are crowded with young. One group of species, the Anis, have become communal in their nesting, several pairs building, tending, and laying in a single nest. The communal group as a unit is territorial, driving off foreign members of the species.

There is ample background then for development of parasitism through enforced scattering of eggs; such pro-

cedure, if successful in yielding young cuckoos, might comparatively soon become the normal pattern of action. Unlike the cowbirds, the breakdown of normal behavior was not loss of territorialism but abandonment of eggs. This type of parasitic evolution has been termed *egg parasitism* to contrast with *nest parasitism* wherein the cowbirds, with diminished interest in territory, became overconcerned with nests and initially stole them for their own use. The end stages, as we have seen, have much in common, although they are not as similar in detail as was once supposed.

Resort to parasitism under several sets of circumstances has not presented great evolutionary difficulties, although its appearance seemingly has depended on the presence of some essential preadaptations that chanced to be available. Having once taken the decisive step and become dependent, the parasitic species may have added refinements, such as rapid delivery of eggs, killing of competing young, and mimicry of egg pattern. In these respects cuckoos are vastly greater specialists than cowbirds, but they probably have been practicing parasitism for a much longer time.

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THE DEVELOPMENT OF THE CONCEPT OF HEAT—II*

FROM THE FIRE PRINCIPLE OF HERACLITUS THROUGH THE CALORIC THEORY OF JOSEPH BLACK

By MARTIN K. BARNETT

Development of the Caloric Hypothesis. As a matter of fact, the caloric theory was scarcely in need of the support coming to it by virtue of the popularity of Newton's emission theory of radiation, for the hypothesis of an imponderable heat fluid was already proving most fruitful in explaining "mixture experiments," as well as the phenomena of liquefaction and vaporization. And if these investigations appear as overwhelmingly important not only for the heat concept but for thermodynamics generally it is precisely because, in them, attention is concentrated on that particular aspect of changes which we now characterize as "their initial and final states," an attempt being made to define the imponderable heat fluid in such a manner that it will appear as quantitatively conserved in the processes in question.

"Mixture experiments" were not new to the eighteenth century; indeed, Renaldini (1694) had already employed mixtures of boiling and freezing water in varying proportions for the purpose of thermometer graduation. However, the mixture experiments of Fahrenheit, carried out at the request of Boerhaave (1732), were viewed from an entirely new standpoint: the thermometer scale was regarded as already given, the final reading of the thermometer being interpreted as indicative of the amounts of heat contributed to the mixture by the constituents. Fahrenheit found that two

equal volumes of water at different temperatures attain, on rapid mixing, a temperature which is the arithmetical mean of the two initial temperatures,¹¹ but that when equal volumes of water and mercury at different temperatures are mixed, the temperature of the mixture is higher or lower than the arithmetical mean, depending on whether water is the warmer or colder constituent. Only when two volumes of water were mixed with three of mercury was the final temperature found to be the arithmetical mean of the initial temperatures.

The standpoint which Boerhaave assumes in attempting to explain Fahrenheit's experiments is typical of the fluid theory adherents. He regards, as a fundamental axiom, the proposition that there exists an imponderable, indestructible fluid which, in the mixture experiments, merely suffers redistribution. The fundamental question then becomes: How was this fluid distributed between the different bodies before mixing? From the viewpoint of historical criticism, Boerhaave's fundamental question is seen to amount to just this: How may the imponderable fluid be defined so as

¹¹ Strictly speaking, if the final temperature is the arithmetical mean of the initial temperatures when one thermometer is employed, it will not be so when another thermometer, employing a different thermometric substance, is used instead, unless the expansion of the first thermometric substance is proportional to that of the second. The appreciation of this fact had to await the exhaustive investigation of thermometric scales by Dulong and Petit (1817).

* Continued from page 172 of preceding issue.

to satisfy the axiomatic requirement of conservation?

Fahrenheit's experiments revealed *one* weight of water to have the same heating effect, per degree change of temperature, as *twenty* weights of mercury. Although his experiments had, at the same time, revealed that *two volumes* of water are equivalent to *three volumes* of mercury, Boerhaave, no doubt preferring to attribute to experimental error the deviation of three-halves from unity rather than that of twenty, sees fit to state that heat is probably distributed between bodies at the same temperature in accordance with their volumes. Thus we see that Boerhaave's heat fluid was endowed with precisely those properties which, more than two thousand years earlier, were attributed to the Eleatic Being, namely, corporeality (space-filling), homogeneity (uniform distribution), and indestructibility.¹²

Confusion of Temperature and Heat.

In the thought of Boerhaave we can detect a confusion of the intensity and capacity factors of heat which was quite prevalent in his day. This had already been evident in the case of Newton (1701), who thought that the "heat" of a hot body must be proportional to its "heat loss" and, at the same time, considered the readings of his actually arbitrary thermometric scale to be a measure of the latter and therefore, he thought, of the former as well. When Boerhaave decides, as a result of the mixture experiments, that heat is probably distributed according to volume, he thinks this view substantiated by the fact that bodies in contact come to a common temperature, thus revealing his confusion between uniform temperature and uniform distribution of heat.

The same confusion in terminology

¹² To be sure, the analogy is not perfect, for Boerhaave did not deny the existence of a vacuum nor that of matter or fluids, other than caloric.

exists in the writings of Richmann (1750), who attempted to give mathematical formulation to the results of mixture experiments employing different quantities of the same substance.¹³ He does not distinguish between temperature and quantity of heat but refers to both as "heat" (calor). On the basis of experiment and theoretical considerations, he stated that the "heat" u of a mass m on being distributed between the masses m and m' yields the "heat" $mu/(m+m')$, so that if two masses m and m' with "heats" u and u' , respectively, are mixed, the resulting "heat" is $(mu + m'u)/(m+m')$. In general, when masses m, m', m'', \dots of "heats" u, u', u'', \dots are mixed, the final "heat" will be

$$U = \frac{mu + m'u + m''u'' + \dots}{m + m' + m'' + \dots}$$

The close relation of Richmann's " mu " product to our "quantity of heat" is obvious. We must also accredit him with a certain insight into the relative nature of his measurements, for he states that it is not the "absolute heats" but only the excesses over the zero point of his thermometer that come into consideration.

From the appearance of masses in his formula, we might suppose that Richmann leaned to the opinion that heat is distributed according to mass. This does not seem to have been the case, although he disproved Boerhaave's assertion that the rate of cooling of a body is inversely proportional to its density by observing that a body of mercury cooled more rapidly, also was heated

¹³ Kraft (1746) had already made an attempt in this direction when he advanced for U , the final temperature of a water mixture, the empirical formula

$$U = \frac{11 mu + 8 m'u'}{11 m + 8 m'}$$

where u and u' denote the initial temperatures of the bodies of water of masses m and m' , respectively. The asymmetry of Kraft's formula must have been due to experimental error.

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more rapidly, than *lighter* bodies of the same size, shape, and initial temperature. However, in his mixture experiments with water, he considered the experimental error due to the heating of flask and thermometer to be eliminated by regarding these as replaced by the same *volumes* of water.

Classic Work of Black. By definitely distinguishing between "heat" and "temperature" and by the introduction of the "heat capacity" and "latent heat" concepts, Joseph Black (1760)¹⁴ probably did more than any other man to convert confusion into order in this most important domain of physical science. He begins, in a thoroughly empirical, yet critical, vein by pointing out that the well-known truth that "all bodies communicating freely with each other, and exposed to no inequality of external action, acquire the same temperature as indicated by a thermometer," is really a remarkable experimental fact which could not have been predicted from any known relation of each of the bodies, separately to heat. This fact, which had previously been referred to as the "equality of heat," Black wisely prefers to call the "equilibrium of heat," since it is not heat equality but temperature equality which is involved. Boerhaave's view, also adopted by Muschenbroek, that two bodies of equal volume and at the same temperature contain the same heats, he recognizes as an instance of confusion between the quantity and intensity factors of heat. Nor is the heat required to raise different bodies through

the same number of degrees proportional to their density. Fahrenheit's experiments clearly show that "the same quantity of the matter of heat has more effect in heating quicksilver than in heating an equal measure (volume) of water. . . . Quicksilver, therefore, has less *capacity* for the matter of heat than water . . . has"; in fact, mercury has only two-thirds the capacity for heat that water has.¹⁵ Heat, then, is distributed in a body, neither according to the body's mass nor its volume but "according to its (the body's) particular capacity, or its particular force of attraction for this matter."

Several investigators on the Continent were led to ideas similar to Black's. With Wilke (1781), the heat capacity notion finds expression in the statement that every body is equivalent to some quantity of water at the same temperature, i.e., the given body will have the same effect as its equivalent quantity of water in raising the temperature of a definite quantity of water through a definite temperature interval.¹⁶

Lambert (1775), like Black, distinguishes between quantity of heat (*Menge der Wärme*) and its intensity (*Kraft* or *Stärke der Wärme*). For bodies of the same substance at the same temperature, the first, he says, is proportional to the volume of the body, but the same quantity of heat has in bodies of different substances of the same volume a different force. Lambert regarded heat as consisting of "fire-particles" (*Feuerteilchen*) and described the Boerhaave-Fahrenheit experiment by stating that three "fire-particles" in water had the same "force of heat" as

¹⁵ Black observes that this fact also explains why, if equal volumes of water and mercury are placed at the same distance before a fire, the mercury warms faster, and when the fire is removed, cools more rapidly, than the water.

¹⁶ To Wilke is due the introduction of the term "specific heat," which was suggested by the analogy with specific gravity (11, ii, p. 159).

¹⁴ Black's posthumous *Lectures on the Elements of Chemistry*, which contained his ideas on heat, was not published till 1803. However, a surreptitious publication, containing an account of his views, appeared in London in 1770. Moreover, after 1760, he disseminated his views among his students orally by means of his lectures (11, ii, p. 161). In this way, many of Black's ideas became dissociated from the name of their originator (4, p. 180).

two "fire-particles" in an equal volume of mercury. From his own observations of the temperature equalization between a liquid contained in a thermometer and a second liquid into which the thermometer was introduced, he concludes that four "fire-particles" have the same heating effect in mercury that six in the same volume of alcohol, and seven in the same volume of water, have.

Concept of Latent Heat. Quite as epoch-making as his ideas concerning heat capacity, was Black's extension of the heat concept to changes of state (liquefaction and vaporization) by means of the notion of "latent heat." When Black first took up the study of these phenomena (around 1760), only one kind of heat was recognized, namely, "sensible heat"; in other words, it was assumed that the addition or subtraction of heat to or from a body must, in every case, make itself evident by a change in the reading of a thermometer in contact with the body. Black demonstrated, with considerable finality, that this view, in the light of observed facts, was quite unscientific and untenable. Thus, he notes that in the case of fusion, if the accepted view be correct, a body of ice at the freezing temperature would be converted, wholly and instantaneously, into liquid by the smallest addition of heat. But the well-known slowness of the melting process shows us that this never happens. To confirm his opinion, Black suspended a body of ice and a body of cold water in a warm room and noted the changes in temperature of each. The temperature of the water rose continuously, but that of the ice rose to the freezing point, remained there until fusion was complete, then again rose continuously. The experiment, says Black, reveals clearly that during fusion heat is absorbed without change of temperature, since the body of ice and the body of water were both receiving heat under exactly the same ex-

terior conditions. This was substantiated when he detected a stream of cold air descending from the ice during fusion, this being correctly described as air which had given up its heat to the ice. On solidification of the melted ice, Black reasoned that the same heat must be liberated which had been absorbed on fusion (Principle of Conservation of Heat). The liberation of heat on freezing was ably proven by reference to the supercooling and subsequent solidification of water accompanied by the rise in temperature of the mixture to the freezing point, a phenomenon known to Fahrenheit. The sudden rise of temperature shows, says Black, that it is not the loss of "sensible," but that of "latent," heat which is the condition of freezing.

From these experiments Black feels himself forced to conclude that the addition of "latent heat" at constant temperature is the "principal and most immediate cause of fluidity," whereas the subtraction of the same is the cause of solidity.¹⁷

Black gives added weight to his views by determining the heat of fusion of ice by two different methods. The first was based on the observation already referred to: a body of ice and a body of water, each at the freezing temperature and of known weight, were placed in a warm room, and from the rise in temperature of the water, the amount of heat absorbed by the ice during fusion was readily calculated. The second method was modeled after the mixture experiments of Boerhaave and Fahrenheit: a small piece of ice at the freezing point was intro-

¹⁷ Some investigators of this time, e.g., Muschenbroek, adopting an ancient view of Democritus, held that water was a fluid not by virtue of heat imparted to it but because of an "essential quality," depending upon the supposed spherical shape of its particles. Freezing, they supposed due to the addition of "frigorific particles," a view supported, in the case of water, by the increase in volume on freezing (4, p. 33).

duced into a large body of warm water and the fall in temperature of the water during fusion noted. This with the weight of the water gave the heat absorbed by the ice which, divided by the initial weight of the ice gave the heat of fusion per unit weight. When we consider that this determination was being made for the first time in the history of physics, Black's value of 77 calories per gram for the latent heat of fusion of ice appears remarkably close to the modern value.

Black advanced exactly similar views to account for the facts of vaporization and liquefaction. Entirely unacquainted with any notion of "latent," as opposed to "sensible," heat many of Black's predecessors had held that the bubbles of gas which are generated in, and subsequently expelled from, a boiling liquid are bubbles of heat itself, and that the temperature of the liquid does not rise simply because this heat passes through the liquid instead of being absorbed. At the same time, others maintained that the bubbles were those of air. Black's careful reasoning, coupled with shrewd observation and cleverly designed experiments brought order into this wilderness of confused ideas. Having identified the bubbles as those of the vapor of the liquid, he shows that the constant temperature during vaporization is consistent with only one assumption, namely, that the absorption of "latent heat" is the cause of the formation of vapor, just as it had been the cause of fusion or liquidity. Indeed, says Black, if this were not the case, the liquid on reaching the boiling point, would, on the addition of the smallest quantity of heat, explode into vapor. Black's analysis of vaporization also explained why hot water, on being brought to boiling by subjecting it to reduced pressure, experienced a sudden and decided cooling, a phenomenon first noted by Boyle.

Black determined the heat of vaporiza-

tion of water and then checked this against a determination of the heat of liquefaction, which, he correctly reasoned, should be equal to the former. The first was determined by steadily heating an initially cold body of water until vaporization was complete. By assuming that the rate of heat absorption during boiling was the same as that before the boiling point was attained, he was able to calculate, from the time elapsing during boiling, the latent heat of vaporization. The heat of liquefaction was measured by distilling a definite quantity of water into a known quantity of cooling water whose rise in temperature was noted. In another experiment the water was distilled into an ice calorimeter and the weight of melted ice noted. Black's first values for the heat of vaporization of water were too low (445, 456 calories per gram). James Watt, his famous student and assistant, introducing several refinements, concluded that the correct value lay between 495 and 525 calories per gram, values which compare favorably with the accurate value, 536, obtained over half a century later by Regnault.

Speculations on the Absolute Zero. Black insisted that hot and cold were purely relative terms and stated that there was no evidence for any lower limit of the temperature scale. Irvine and Crawford (1778), on the other hand, regarded the fusion experiments as supplying data for the determination of the absolute zero, i.e., the temperature corresponding to the state of zero heat content. Instead of regarding the latent heat of fusion as simply the direct cause of fluidity, as Black had done, they reasoned that, since the heat capacity of water is greater than that of ice, the latent heat must represent the net excess of total heat of the water at the freezing temperature over that contained in the ice at the same temperature. This as-

sumption, coupled with the supposed invariability of heat capacity with temperature, provided all that was necessary for the calculation of the absolute zero of temperature. Thus if c and c' be the heat capacities (specific) of water and ice, respectively, and the absolute zero be t degrees below the freezing point, the view of Irvine and Crawford requires that Q , the heat of fusion, be given by

$$Q = tc - tc'$$

Substituting $Q = 80$, $2c' = c$, we obtain $t = 160$, i.e., the absolute zero is at -160° C. Further, this view requires that the heat of fusion decreases with decreasing temperature. Thus, for ice, if T be the Centigrade temperature, we have $Q = \frac{1}{2}ct = \frac{1}{2}(160 + T)$.

Dalton, who also adopted this view, showed that the same reasoning, applied to the heat of fusion of mercury, gave the absolute zero as -2021° C. Gadolin, adopting similar assumptions to account for the heat developed on mixing two liquids at the same temperature, found that the heat of solution of sulfuric acid in water indicated the absolute zero to be between -830° C. and -1720° C. Still other "absolute zeros" were calculated from other mixture experiments and from the heat of chemical reactions.¹⁸

Specific Heat Determinations. Through introduction of the concepts heat capac-

¹⁸ As Mach (10, p. 168) notes, even though the assumption that the heat of fusion represents the net excess of the heat of the liquid over that of the solid at the freezing temperature, along with that of the invariability of heat capacity with temperature, be accepted, still the data need not necessarily be regarded as indicative of an absolute zero: if the temperature calculated be assumed to be that at which the heat of fusion in question assumes a zero value, then one may assume, equally well, that below this temperature the heat of fusion acquires negative values. Indeed, such a point of view would have eradicated the apparent discrepancies between the different "absolute zeros," since there would be no reason for supposing that all substances exhibited a zero heat of fusion at one and the same temperature.

ity and latent heat, Black provided a secure theoretical basis for the measurement of quantities of heat, and hence specific heats, by two distinct methods, the method of mixtures and the method of melting ice. Black himself determined a number of specific heats, but most of the determinations date from those of Irvine (1763), Black's student, who measured the specific heats of mercury, glass, iron filings, and numerous other substances by the method of mixtures.¹⁹

Wilke (1772) employed the ice calorimeter for the measurement of specific heats as also did Laplace and Lavoisier (1780). To the latter is due the recognition of the general variation of specific heat with temperature and the realization that the specific heat can be precisely defined only by means of the calculus. They thus wrote: $s = dQ/dt$. They are with Black in refusing to assume any absolute zero of temperature, nor do they accept the view that the heat of a chemical reaction is to be attributed generally to the difference in heat capacities of reactants and products.

The first accurate measurements of specific heats were those of Dulong and Petit (1813), who employed the water calorimeter. They confirmed the variability of specific heat with temperature, already observed by Laplace and Lavoisier. They also discovered (1819) the law, for which they are best known, that

¹⁹ Let m and m' be, respectively, the mass of the body introduced and the water value of the calorimeter in grams of water. Then if the initial temperatures of body and water calorimeter are t and t' , respectively, whereas the final common temperature is t'' , we have

$$m(t - t'')s = m'(t'' - t')$$

from which s , the specific heat of the substance composing the body in question, is determined.

Again, if a body of mass m , specific heat s , and temperature t causes by its introduction into the ice calorimeter the melting of μ grams of ice, we have

$$mst = 80\mu$$

from which s may be calculated.

the atomic heat capacities of most solid elements have approximately the same value, namely, about six calories. A similar generalization for the molecular heats of certain classes of solid compounds was later discovered by Neumann (1831).

The determination of the specific heats of gases have a special significance for the history of thermodynamics because it was to bodies in the gaseous state that the first and second laws were first developed analytically. The first attempts in this direction were those of Crawford (1778) who introduced heated metal cylinders, containing weighed quantities of the gases, directly into a calorimeter. The results were unsatisfactory because of the relatively small weights of gas employed and the consequently small heating effect of the gas.²⁰ Laplace and Lavoisier (1784) remedied this by sweeping a large quantity m of gas through the coiled tube of an ice calorimeter, noting the fall of the temperature of the gas θ and the weight μ of ice melted. Then $ms\theta = 80\mu$, where s is the specific heat of the gas.

Clement and Desormes (1819) filled the same flasks with different gases, in turn, at the same temperature and pressure, and introduced it into the water calorimeter, whereupon the heat capacities of these masses of gases were placed proportional to the times required to warm the gases through the same temperature interval.

The first accurate measurement of specific heats of gases were those of Delaroche and Berard (1813). The gas at temperature u_1 and constant pressure was led at the rate of m grams per minute through the coiled tube of a water calorimeter where it was cooled to u_2 . If a steady state has been reached, indi-

cated by a constant temperature of the calorimeter, the latter must be losing heat to the surroundings at the same rate at which it is absorbing heat from the gas. Hence, if the water value of the calorimeter (at the constant final temperature) be w and if the calorimeter is known to cool at the rate of $v^\circ\text{C.}$ per minute, when no gas is supplied, then

$$ms(u_1 - u_2) = wv,$$

where s is the specific heat of the gas.

Delaroche and Berard's experiments indicated that different gases have different heat capacities, but Haycraft (1824), refining their methods, thought he could conclude that equal *volumes* of different gases at the same pressure have equal heat capacities, a view supported later by Delarive and Marcet (1827). However, the most careful investigations of Regnault (1837), by the method of Delaroche and Berard, showed this to be true only for oxygen, hydrogen, and nitrogen, whose specific heats he also found to be independent of temperature. He was not able to detect any variation in the specific heats (unit weight) of gases with variation in pressure.

We must note that some of the above methods yield the specific heat at constant volume, others the specific heat at constant pressure. The early workers in this field did not clearly appreciate the significance of this distinction, and the confusion arising from attempts to explain the discrepancy involved, along with that of the cooling of a gas incurred on expansion as well as its heating on compression, constituted a principal factor in the fall of the caloric theory and the enunciation of the First Law of Thermodynamics.

Heat of Chemical Reaction. However, in the meantime, the caloric hypothesis was also demonstrating its great service in other branches of the phenomena of heat. Lavoisier, in researches carried out with Laplace on specific heats and

²⁰ Thus Crawford found, for the specific heat of air, 2 calories per gram(!). Lavoisier and Laplace, with their much improved method, got 0.33, the correct value being 0.2374.

latent heats, had further demonstrated the great scientific value of these concepts, and he attempted to extend the idea of latent heat to account for the heat of chemical reactions. In this line of thought, caloric (*calorique*), though admittedly imponderable, nevertheless plays somewhat the role of a chemical element.²¹ Since to convert solids to liquids, and the latter to gases, a great amount of caloric must be supplied, Lavoisier supposed that gases, air in particular, contain the greatest amount of caloric. Accordingly, when a solid burns in air to form a solid oxide, Lavoisier supposes the caloric liberated to be that which was latent in the air, and states that the heat of combustion must be greatest when two gases combine to form a solid. He thus associates the liberation of caloric in a chemical reaction with the change of state of aggregation occurring (12, p. 27). However, in the case where both reactants and products are gaseous, he is forced to explain the heat effect as due to the difference in specific heats of reactants and products.

The conviction that caloric is a conservative quantity led Laplace and Lavoisier to assert that the heat liberated when a system changes its state is equal to that consumed when the system goes through the reverse change, a generalization of the principle employed by Black in dealing with fusion and vaporization. This generalization, which La-

²¹ Light (*lumière*) and caloric (*calorique*) appeared at the very top of Lavoisier's table of elements (*tableau des substances simples*), followed by the chemical elements known in his day. As older terms for caloric, he lists heat, principle of heat, igneous fluid, fire, matter of fire, matter of heat, but not phlogiston. Yet, in expounding his theory of combustion (1777), he states that caloric, as well as light, are evolved in every combustion, quite in the manner of the phlogisticians (17, p. 55). The essential point here is, of course, that Lavoisier recognizes that the liberation of the *imponderable* caloric is only one aspect of combustion and is accompanied by combinations with the *ponderable* oxygen of the air.

place and Lavoisier were able to verify experimentally to a certain extent, later attained great significance in the work of Hess (1840) where it assumed the form that the heat liberated in a chemical change is independent of the path of the change.²²

The caloric theory also served to clarify the phenomenon of cooling. As early as 1740, Martine had discovered deviations from Newton's Law of Cooling and these were verified by Kraft, Richmann, Leslie, and Dalton; in fact, the latter had proposed a new temperature scale for which Newton's law would be exact. The exact investigation of the rate of cooling of a body was first carried out by Dulong and Petit (1817). Whereas Newton's treatment had been obscured by the confusion of temperature and heat, Dulong and Petit, profiting by the advances instituted by Black, clearly distinguish between these two concepts. Adopting the fundamental notion of heat as a conservative quantity, they regard the net heat lost per unit time by a cooling body as equal to the excess of the heat radiated over that absorbed per unit time. The rate of heat radiation and absorption was, in turn, recognized as being dependent on the temperatures of body and surroundings, on the form and extent of surface of the cooling body, on the total mass of the body, on the peculiar nature of the surface of the body (coefficient of emissivity), and on the nature of the surrounding medium (4, p. 504).

Caloric and Heat Conduction. Another great support of the fluid theory

²² The influence of Lavoisier's relegation of caloric to the same class as the chemical elements is seen in Hess's attempt to extend the law of multiple proportions to caloric, i.e., he tried to show that "when two substances combine in several proportions, the quantities of heat which are produced in the formation of the different compounds stand to one another in multiple proportions" (13, p. 504).

was found in the firm foundation it provided for the theory of heat conduction which attained its highest stage of development in the hands of Fourier (1822). The earliest problems in heat conductivity seem to have been associated with the variation in temperature at the different points of an iron bar subjected to a constant source of heat at one end. The first attempt at a quantitative treatment was that of Amontons (1703), who thought that the temperature increased linearly with the distance from the cold end. To Lambert (1778) is due the realization that the "steady state" attained by such a bar is really a dynamic one in which the heat gained by any element from the hot end equals the sum of the heat given up by the element to the surrounding air and that passed on to the colder part of the bar.

The confusion of radiated heat and conducted heat was naturally associated with the confusion of emissivity and conductivity (often referred to as "outer," or "surface conductivity," and "inner conductivity," respectively). Thus Franklin supposed that, if bars of different metals, but having the same dimensions, be coated with wax and all exposed, at one end, to the same source of heat, then the distances over which the wax was melted, in the steady state, would be proportional to their conductivities for heat. Ingenhouss (1785) adopted this point of view and carried out the experiments. But J. T. Mayer (1791), reasoning that the best conductors of heat should be those which lose their heat to the air most rapidly, concluded that Ingenhouss' best conductors were actually the worst.

The first sound theoretical treatment of the stationary state of a unilaterally heated bar is due to Biot (1804). He reasoned that, at any point, the heat gained from the hot end equals that lost to the air throughout the remainder of the bar, which, in turn, may be computed

from Newton's law of cooling. In this way, he deduced the law that, for distances from the cold end which form an arithmetical progression, the corresponding temperature excesses over the surroundings form a geometric progression, also verifying it experimentally over a considerable temperature range.

All of the laws of heat conduction were developed in a marvellously systematic and comprehensive way by Fourier, working from 1807 to 1822, when his great *Théorie analytique de la Chaleur* was published. He distinguished clearly between heat capacity, emissivity, and conductivity, and formulated the first precise analytical definition of the latter concept. His whole theory of conduction followed rigorously from a simple fact, taken as first principle: the quantities of heat exchanged between two parts of a conducting body, lying very close together, are proportional to their temperature difference. The solution of the problem of the distribution of heat in a long bar in the steady state appeared as a mere detail in Fourier's comprehensive elaboration: he showed that, assuming invariability of conductivity with temperature, the distances over which the wax was melted in Ingenhouss' experiment were proportional not to the conductivities themselves but to their squares.

Although Fourier does not commit himself on the nature of heat, the latter concept appears in his equations precisely as if it were an imponderable fluid which suffers change of distribution but never gain or loss in total quantity, and which strives to attain a uniform "temperature level" just as a ponderable fluid seeks a condition of uniform height above sea level. The fluid theory of electricity had already been advanced in the eighteenth century by Franklin, Aepinus, Coulomb, and others; and Ohm (1826), impressed by Fourier's work, conceived of the analogy between flow

of electricity and flow of heat. He argued that an analogue of the temperature difference of Fourier's theory should exist for electric currents and was thus led to formulate that most useful concept, electromotive force ("electroscopic force").

Caloric as Substance. With so many victories to its credit, it is not surprising that the caloric theory came to occupy a position of commanding influence and relative security, a position which it did not completely relinquish until the middle of the nineteenth century. In particular, the growing number of facts consistent with the caloric hypotheses had the effect of bringing increasingly into prominence the conviction that caloric was an actual substance.

This is already evident in the terminology of Boerhaave who, by referring to heat by the Latin term *ignis*, reveals the close relationship between the eighteenth century heat fluid and the Greek "fire-matter."

It is significant that Black, who carefully warned scientists against attempts at ultimate explanations in terms of hypothetical abstractions (15, pp. 282-285), nevertheless revealed, in his terminology, a firm belief in the substantial nature of caloric. For he refers to heat as "matter of heat" and characterizes the "equilibrium of heat" by the very materialistic phrase "equality of saturation." Moreover, adopting Cleghorn's assumption that caloric exhibits an attractive force for different substances, he identifies the heat capacity of a body as "its particular force of attraction for this matter."

Berthollet, to whom Ellis (2, p. 186) attributes the introduction of the term caloric, cautiously defined it simply as the "cause of heat." Lavoisier was, at times, equally careful, for he says (2, p. 186), "rigorously speaking, we are not even obliged to suppose that caloric

is a real substance; it suffices that it may be any cause whatever which separates the molecules of matter, and we can thus consider its effects in an abstract and mathematical way." Yet the phraseology of this very excerpt ("we are not even obliged" to consider it a substance) indicates that Lavoisier considered it both natural and legitimate, even though unnecessary, to regard caloric as a "real substance." That this was indeed his view is corroborated by his explanation of the explosive force of gunpowder: this, he said, is due to the sudden liberation of caloric, "that highly elastic fluid," also by his reference to fusion as a process of "solution in caloric." But the most convincing evidence of his belief in the substantial nature of heat is found in the circumstance that even after he had, by his investigation of combustion, banished phlogiston from science, he nevertheless continued to retain "caloric" in his table of chemical elements.

Cleghorn and Black had endowed caloric with the fundamental Newtonian property of matter, that of attraction (for other kinds of matter), and Lavoisier had even placed caloric in his table of elements. From these circumstances, it is not surprising that some eighteenth century calorists thought that caloric possessed at least *some* mass (4, p. 32). Otherwise Rumford, a great opponent of the caloric theory, would hardly have considered it worth-while to carry out his investigation, *An Inquiry Concerning the Weight of Heat* (1799). He was able to show conclusively that caloric, if it exists at all, does not possess mass.

After Rumford's investigations, all calorists recognized that caloric, like electricity and magnetism, must be an imponderable. However, the property of weight had never been regarded as fundamental, except insofar as it was implied by the postulated attraction of caloric for matter. A more fundamen-

tal property, which had been assumed from the beginning, was that of the elasticity and self-repellent nature ascribed to the heat fluid.²³ Thus Dalton (16, ii, p. 393) says that besides the force of attraction between particles, "we find another force that is likewise universal, . . . a force of repulsion. This is now generally, and I think properly, ascribed to the agency of heat. An atmosphere of this subtle fluid constantly surrounds the atoms of all bodies and prevents them from being drawn into actual contact." Also Fourier speaks of "the equilibrium which exists, in the interior of a solid mass, between the repulsive force of heat and molecular attraction." He also says that "heat is the origin of all elasticity."

This self-repellent property of caloric explained why the volumes of bodies quite generally increased on heating (at constant pressure). The anomalous cases of the fusion of ice and the contraction of water between the freezing point and 4°C. were "explained" by reference to analogous cases in mixtures of ordinary matter; e.g., the solution of water and alcohol, also that of copper in tin, and numerous chemical reactions are accompanied by a decrease in volume.

Indeed, the magnificent edifice comprising both theory and experiment, erected on the basis of the caloric hypothesis, must cause us to place it among the most fruitful hypotheses in the history of science. However, in the meantime, the facts of frictional and electrical generation of heat and the heating and cooling of gases on compression and ex-

pansion, respectively, were coming into increased prominence and were destined, after a battle of half a century, to bring about the fall of the fluid theory. This fall was coincident with the establishment of the First Law of Thermodynamics and, in many cases, with the revival of the kinetic theory of heat.

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²³ As early as 1733, the same property had been attributed to each of the two electric fluids by Dufay (11, ii, p. 201).

BASIC ENGLISH FOR SCIENCE

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THE following letter (evidently not written to be posted) I found in the English notebook of one of my students in Freshman Composition:

TO THE AUTHORS OF *A Textbook of Botany*:

As a student in the botany course which is given at our university, I have become familiar with your textbook and workbook which are used in connection with the course. I wish to inform you that I am having a great deal of difficulty with botany, and I believe your books are largely responsible.

The purpose of botany, I believe, is to acquaint the student with the different types of plant life and to help him understand the growth and structure of plants. In my estimation *A Textbook of Botany* defeats that purpose. The average student is lost in the maze of difficult and highly technical language of your text and in the complexity of the demonstrations and problems in your workbook. For example, in describing the beginning of a leaf you state that "development of a leaf begins with the proliferation of a primordium"—without any previous hint of what a primordium is!

As a result of your heavy treatment of the subject, botany is dreaded and disliked by the majority of students on this campus. Many have failed the course because of this dislike—for which your boring textbook is largely responsible. You have, in the eyes of many students, attached a stigma to the useful science of botany.

The evident sincerity of this letter, I hope it will be agreed, entitles it to a fair hearing. What college freshman has not at some time or another felt a similar protest rising within him as he tried to advance through the maze of language between him and the subject he was studying? The writer of the above letter may pass her course in botany and give the lie to her fears. She may even go on to like botany. But what a pity that she must arrive in spite of the language in which her textbooks are

written. Much has been said on the teachers' side of the difficulty instructors of physics, of botany, and of chemistry have in getting their students interested in these branches of science. Perhaps the main reason lies not in reason mainly is not the students' dislike for the subject itself but for the language in which the subject is presented to them.

Too many college texts in science are burdened with an unnecessarily heavy style. The use of essential scientific words makes for economy; certainly the author is not expected to eschew them to the point of repeating long definitions. But why cannot he occasionally use "growth" instead of his beloved "proliferation"? "Chain of events" instead of "series of concatenations"? "Scaling off" instead of "desquamation"? It would seem that some authors of secondary science texts think that unless they write in the style of Herbert Spencer's definition of evolution, they cannot impress their readers with the importance of their subjects; as if what is stated simply cannot be worth learning. Clear exposition is a craft which scientific writers ought to regard as highly as the validity of their ideas. Generally speaking, they seem not to be aware of its existence; or, if they are, acknowledge it by keeping as far as possible from it—after the example of Professor Longbore, who used to open his science lectures each quarter with this warning, the only intelligible sentence in his discourses: "I do not intend to make clear to you in twelve weeks what it took me fifty years to learn."

The style of Professor Longbore and his ilk is probably the result of a passive rather than an active state of mind. As

one turns the pages of a ponderously written text in college zoology, for example, he begins to wonder whether the author may not have drifted into his style merely by following the course of least resistance. A polysyllabic style is a lazy style. It is easy to master the learned jargon of any science, and mastery of the jargon is too often mistaken by publishers' readers for mastery of the subject. "Easy writing makes cursed hard reading," observed Dick Sheridan; and although laborious writing is not guaranteed *per se* to make easy reading, it has a good chance to, if the writer knows what he wants to say and tries hard enough to say it. My point is that it is downright hard work to express scientific concepts in a clear, mature style. And yet texts written for college students ought to be worth that much effort.

For some writers, no doubt, there is a fascination in the weighty language of which my student complained. Thus the trap is baited and set for the author's complete undoing: he lets words take the place of thought. He has seen these splendid terms so often; they were right to him in the books he read. Are they not as good in his own? He does not stop to ask what the words really mean, how he expects his reader to interpret them. If by any chance a conscientious student narrows his eyes and carefully examines this lingo, the result is usually a feeling of dismay like that expressed in the letter at the beginning of this article.

Is there, for example, any reason why a book in psychology should be written in this style?

The apperception of self-motivation is a psychological fact. A concomitant phenomenon is the consciousness that the origin of this motivation is internal and not external.

Is not this what the writer *means*?

The mind is conscious that it is self-moving; and at the same time, that the motion comes from within itself.

The last sentence above is written in Basic English. This simplified English ought to have an especial appeal to scientific writers because its discovery was analogous to the procedure of the scientist seeking basic principles in the natural world. The originators of Basic English, sifting the thousands of words in our language, isolated 850 indispensable terms by which the meanings of the others could be expressed. For science an additional list of 100 words is provided.

The methods by which the Basic word list was determined can be tested by anyone who takes a dictionary in his hand. He will find in reading definitions that certain words keep returning time after time—usually little words such as *go, get, make, be, thing, name, true, good*, together with necessary conjunctions and prepositions. These words and others of their kind *are* the basic vocabulary of our language. They make a restricted common ground on which it is possible for writer and reader to meet with the least possible chance for confusion or mistake. In its inductive origin, as well as in its purposes, Basic English is scientific English.

It is not urged here that all writers of college texts in science adopt at once the Basic English vocabulary. The Spartan simplicity of Basic, though it is the handmaiden of truth, does not always serve other ideals as faithfully. Variety and subtlety, for example, are not main properties of Basic. These virtues and other qualities of a pleasing style ought not to be lacking from the books our science students read. Nevertheless Basic English could have a tonic effect upon these books. It could dispel much foggy thinking, which is the real cause of bad writing. If an author *thought* in Basic first, he would not write "heliotropic inclination toward the illuminating source." He would see that the meaning of his first word is repeated needlessly in the

five that follow and might decide that his whole phrase could be put thus: "turning in the direction of the light"—which is good science and good Basic. No one can compose in Basic without having in his mind a pretty clear idea of what he wants to say. There are no superfluous terms in Basic to get between him and his manuscript. He will often be reminded that between his idea *A* and the words *B* that represent it there ought to be the same relation as between an object *a* held before a mirror and its reflection *b*. A true reflection requires a good mirror. Basic English has the makings of a good mirror because its vocabulary is level and impersonal—a plane reflector. Even though the scientific writer makes use of a larger vocabulary, if he keeps firmly in mind Basic equivalents as he composes his sentences, his writing will gain clearness, whatever words he finally chooses. And his readers—his students or his peers—will call him blessed.

BUT there is another field of scientific writing where the need for Basic English is far more pressing. I mean the scientific books and magazines printed in this country and Great Britain. A great many foreigners before World War II were coming into English via Basic. Now as an international language Basic is gaining steadily in general esteem everywhere. Public interest in it was greatly stimulated by Winston Churchill's ardent approval of Basic at Harvard in 1943. No artificial language can meet the stern needs of an international tongue as Basic English can. First, it has behind it the compelling prestige of the Anglo-Saxon tradition; it "looks" like English and it is English, the vital heart and core of the language of Shakespeare and Jefferson. Basic is easy for the non-English speaker to learn. A few weeks' steady effort under skilled direction can make an intelligent foreigner at home in written and spoken

Basic. The demand for books in Basic, both here and abroad, is on the upswing. It is one sign of the world-hunger for unity and commonalty among the peoples of our shrinking planet.

In satisfying this hunger the place of science is nothing less than strategic. It remains for science to recognize some of the practical aspects of its position. Science, as an international agency, must create or adopt an international tongue. The scientist today is faced with the problems faced by English traders 500 years ago as they carried their goods and their language into the Seven Seas. Through necessity, between them and their brown-, black-, and yellow-skinned customers, a species of international language slowly developed. The barbarous pidgin ("merchant") English of the Far East is a natural phenomenon brought into being by the needs of men groping toward each other's minds. These needs are a hundred times more imperative today. The very existence of the race may depend upon our finding right answers to them. Science, like trade, now has the earth as its province. More fortunate than trade, science does not have to await the development of a crude, mass-made English. A scientifically evolved speech is at hand; in the words of Mr. Churchill, "a very carefully wrought plan for an international language, capable of very wide transactions."

It is a truism to say that the great impetus felt by scientific research during the past five years will continue and accelerate. Parallel with this step-up of activity in the ranks of the scientists is a keen public concern about what they are doing. Jet-propelled aircraft and atomic bombs have drawn the fearful attention of everyone to the laboratory of the technician. This public interest cannot be written off as mere curiosity. We are hearing it said on all sides: Why, if the scientist is so expert in devising the

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machines of death and destruction, why cannot he turn his talents as effectively to the service of humanity? This protest is admittedly naïve: Burbank and Edison were scientists. But the protest still stands. Its ultimate meaning is that everyone the world over wants to know what the scientist is about.

Modern science has therefore a vast new social responsibility which it cannot ignore. The day of unadulterated "pure" research is about over. Even though the scientist may not, like Terence, agree that "Everyman's business is my business," Everyman is telling the world and himself that "the scientist's business is my business." And Everyman pays the taxes and makes the grants that keep the scientist going. Everyman is a Chinese farmer, a Chicago businessman, a French taxi driver, a Greek fisherman, a Russian fur dealer. All these are invading the hitherto sacred confines of the technician's laboratory. And they have a right to do so.

In practical terms this means that the findings of the technician must be put on paper. Books must be written, articles contributed to scientific and lay journals. At present the chances are twenty to one that the native tongue of our hypothetical scientist will be English. Why should he not address himself to his world-wide audience in a truly international language—Basic English?

Basic is surprisingly easy for the English user to learn. With a little experience a copy writer can translate a full-English draft into Basic about as rapidly as he can compose. It is most desirable, of course, that the scientific writer prepare his own Basic version of his books and articles. Thus the thoughts of such authorities as Sir James Jeans, J. B. S. Haldane, Walter S. Landis, and Sir Arthur Stanley Eddington could go directly to the minds of men all over the earth without the warped meanings and false emphases that lurk in translations.

In facilitating this direct communication between the writing scientist and his universal reader, the American and British scientific journals have a place of unique importance. Their large circulation is a token of the immense service they can render to science and to humanity. By the use of complete articles in Basic English and by special Basic editions and supplements, they can directly interpret the findings of modern science to a circle of readers that in a very true sense is world-wide. In so doing they will be assuming their share in the large responsibilities borne by science in the world today.

I wish to conclude by submitting a brief specimen of Basic translation. The original, which follows, was chosen from Sir Charles Lyell's well-known *Progress of Geology*:

(1) For more than two centuries the shelly strata of the Subapennine hills afforded matter of speculation to the early geologists of Italy, and few of them had any suspicion that similar deposits were then forming in the neighboring sea. (2) Some imagined that the strata, so rich in organic remains, instead of being due to secondary agents, had been so created in the beginning of things by the fiat of the Almighty. (3) Others ascribed the imbedded fossil bodies to some plastic power which resided in the earth in the early ages of the world. (4) In what manner were these dogmas at length exploded? (5) The fossil relics were carefully compared with their living analogues, and all doubts as to their organic origin were eventually dispelled. (6) So, also, in regard to the nature of the containing beds of mud, sand, and limestone; those parts of the bottom of the sea were examined where shells are now becoming annually entombed in new deposits. (7) Donati explored the bed of the Adriatic, and found the closest resemblance between the strata there forming, and those which constituted hills above a thousand feet high in various parts of the Italian peninsula. (8) He ascertained by dredging that living testacea were there grouped together in precisely the same manner as were their fossil analogues in the inland strata; and while some of the recent shells of the Adriatic were becoming incrustated with calcareous rock, he observed that others had been newly buried in sand and clay, precisely as fossil shells occur in the Subapennine hills.

Basic English

(1) For more than 200 years the shell layers of the small mountains near the Apennines had been a question for discussion among the persons in Italy who first became interested in the science of the earth's history as recorded in beds of rock. (1a) Only a very small number had any idea that like deposits were then forming in the nearby sea. (2) Some had the idea that the rock layers, which had in them a great amount of the dead substance of things once living, had been made not by the decomposition of those things, but by an order of the Almighty when He made the earth. (3) Others gave the explanation that the plant and animal bodies in the stone beds were deposited there by some force of swelling and contraction which was in the earth in its early days. (4) In what way was the demonstration made at last that these ideas were false? (5) An exact comparison was made between the stone plants and animals and the living ones like them, and all doubts that the

stone forms had come from living forms were put away at last. (6) The same fact was made clear about the substances in the sea-beds of earth, sand, and *lime* stone (stone having a great amount of chalk): tests were made of those parts of the sea-floor where shells are now year by year being covered over in new deposits. (7) Donati took samples from different parts of the bed of the Adriatic and made the discovery that the layers forming there were very much like those which made up small mountains over 1,000 feet high in different parts of Italy itself. (8) He made the discovery by taking up samples from the seafloor that living *testacea* (a species of small shell-covered animals) were there grouped together in exactly the same way as were their like stone forms in the inland layers; and at the same time some of the new shells in the Adriatic were being covered with *lime*-stone rock, he took note that others had been newly covered by sand and sticky earth, exactly as the stone-covered shells were, in the small mountains near the Apennines.

POSSIBILITY

*If grass is green beyond the galaxies
Beneath blue skies of undiscovered spheres
Where other beings see their hopes and fears
In stars that haunt their own mythologies;
If other worlds are rimmed by sounding seas
Endlessly moving over measured years,
And eyes of men beyond their brim of tears
Are shining into distant mysteries;*

*If another earth is near a sun unknown
Among the hinterlands of stars unfound,
The colors of its captive dreams may be
Within the shattered spectrum of our own,
Its silent fantasies of inner sound
The voices of our own eternity.*

JOEL W. HEDGPETH

SCIENCE ON THE MARCH

SAINT LOUIS TODAY

IT IS A pleasure for the cultural institutions of St. Louis to serve again as hosts to the members of the American Association for the Advancement of Science, March 27-30, 1946. In spite of many handicaps of the past four years a considerable degree of progress has been achieved by the industrial, social, and educational organizations of the city. Those scientists who attended the meetings that were held here in 1935 will find new points of interest and notable additions to the old ones.

Opposite the entrance to Union Station the Milles Fountain group, symbolizing the confluence of the great Mississippi and Missouri rivers a few miles north of the city, now offers a decidedly more pleasing greeting to visitors than the unsightly buildings that formerly existed on the site.

Immediately prior to the war many

blocks of the older river-front buildings were demolished in preparation for a great memorial plaza. The war interrupted these plans, but enough has been accomplished to show that on their completion St. Louis will stand out as one of the most beautiful riverside cities in the world. And to those who have not visited this Midwestern city during the past ten years the most striking change will be its cleanliness. With the passing of an effective antismoke ordinance St. Louis now ranks with the tidiest of America's large industrial communities.

St. Louis and Washington universities will be points of most immediate interest aside from the scheduled meetings in the Kiel Auditorium and various hotels. At Washington University, Brown Hall, housing the School of Social Studies, was opened in 1937, and presents an attractive architectural as well as educational addition. In a special building near



THE BARNES HOSPITAL GROUP NEAR FOREST PARK
St. Louis Chamber of Commerce

Crow Hall the cyclotron that played an important part in the earlier phases of wartime atomic research should not be overlooked by those attending the physical science meetings.

St. Louis University, located in the heart of the city, is especially noted for its work in the fields of medicine and seismology. It was, in fact, the first institution of learning in the world to establish a department of geophysics and is still the only university in the United States to have a separately organized department of this kind. By means of an elaborate system of seismographs, strategically placed at five points in Missouri and Arkansas, the University "keeps its finger on the pulse of the earth." In the field of medicine the work of the recent Nobel Prize winner, Dr. Edward A. Doisy, is representative of the great

strides that have been made of late years in the Medical School of this University.

March is not an especially favorable time for outdoor plantings at the Missouri Botanical Garden (Shaw's Garden), but the greenhouses in the city garden show many improvements over ten years ago, and a special floral display will be presented at the time of the Association meetings. The library and herbarium will attract those whose activities center in the more technical phases of the botanical sciences.

The most evident progress at the Missouri Botanical Garden during the past decade is in the development of the Arboretum at Gray Summit, on the northern fringe of the Ozarks and overlooking the Meramec River. The entire orchid collection of the Garden, numbering some 20,000 plants, is now housed in green-



Day Photographs

BROWN HALL, WASHINGTON UNIVERSITY'S NEWEST BUILDING

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MAIN GREENHOUSE, MISSOURI BOTANICAL GARDEN

houses on the Arboretum grounds. During the past four years outstanding research has been carried on dealing with the light relations and the hybridizing of orchids, and with the technique of hydroponics in their culture. Variations of this method are now employed on a large scale.

For those with a few hours of time from busy meetings the municipal Forest Park of 1,400 acres offers a unique assemblage of attractions. The more serious-minded will appreciate the Art Museum where there are constantly changing exhibits covering many phases of art, as well as the large and splendid permanent collections. On the north side of the Park, directly opposite the Art Museum, is the Jefferson Memorial. This is occupied in part by the fine library of the Missouri Historical Society, containing a large collection of Jefferson manuscripts and others pertaining to the

Hamilton-Burr controversy. And during the past decade the Lindbergh Collection has attracted many millions of visitors.

Only a short distance from the Art Museum are the Zoological Gardens, housing a magnificent collection of animals from all corners of the earth. In recent years the Zoo has improved in the acquisition of rare animals as well as in landscaping and in the enlargement of the houses. The reptile and bird houses are acclaimed as the finest in the world—zoological treasures to the professional student and layman alike.

The Park also includes a magnificent modern greenhouse, known as the Jewel Box, which presents exquisitely arranged floral displays according to the season. With the advent of spring, at about the time planned for the meetings, Forest Park should be given high priority on the visitor's list.

*Famous Barry Co.*

ADMINISTRATION BUILDING, ST. LOUIS UNIVERSITY

For the historically-minded who have an hour or two to spare a number of places may be visited which hark back to the glamorous river days of the early nineteenth century, when St. Louis was the chief embarkation point for the western caravans that rumbled over the high plains to the forests of the Pacific Coast.

The Old Cathedral and Court House, both constructed in the 1830's, are only a few blocks from the downtown meeting points of the Association and are rich in the historic lore of the river-front days of a century ago. The Campbell House and Field Home are also representative historic landmarks. The former, built by Robert Campbell, a fur trader of the 1820's, contains the original furnishings and is typical of a wealthy home of the period. The Field Home, where the poet

Eugene Field was born in 1850, is located near the downtown business district. It has been rehabilitated and refurnished with articles used by Field in his earlier days.

Space does not permit even a listing of the many industrial plants that might be of interest to the varied scientific callings represented among the Association's members. However, particularly noteworthy for biologists are the many breweries for which this city is justly renowned. Thousands of persons are conducted through these plants each year, thus being afforded an opportunity to see the most modern techniques and applications of intensive research in industrial microbiology.

HENRY N. ANDREWS

MISSOURI BOTANICAL GARDEN

LET'S HEAR ABOUT IT

THERE is an old saying among otologists (ear doctors) that one should not put anything in his right ear except his left elbow. The advice, if followed, would prevent many a ruptured eardrum.

The Navy, of course, has more than an academic interest in the hearing of its personnel. Before a man is asked to sign on the dotted line, he is given a pseudoscientific test in the form of a whisper, tick of a watch, or the click of a coin. Before he is handed his honorable discharge, he is tested again with the same pseudoscientific test, and it is hoped that his hearing has not been impaired to the extent of furnishing, at a later date, a basis for claims against the Government.

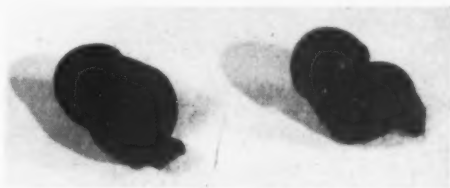
It may be that the prefix "pseudo" will raise unpleasant connotations, but any test which ranges from the dulcet whisper of a corpsman from South Carolina to the gravelly rumble of a corpsman from Maine certainly is not scientific. Likewise, the click of a coin or the tick of a dollar watch can hardly be trusted. However, there is no known record of any man in the Navy having hearing so bad that he could not hear the call to chow.

After a man has entered the service, his hearing is subjected to many influences. In addition to the ordinary noises that he encountered in what is euphemistically called civilian life, he is exposed to the noise and blast effect of the guns and, in certain ships, to the high noise level of the Diesel engine rooms. The effect of the guns upon hearing is not open to argument; too many ruptured eardrums offer their eloquent testimony. The effect upon the hearing of personnel being subjected to the high noise level of Diesel engine rooms is questionable. The specialists cannot seem to come to an agreement as

to whether there is such a thing as occupational deafness.

It would seem to the layman that continual exposure to high noise levels would eventually have a deleterious effect upon hearing. A number of medical men subscribe to this theory. Another group who venerate statistics maintain with more heat than light that, as we have no scientific test data on the hearing of these men before they were subjected to the high noise level, we have no means of knowing whether the loss of hearing was occupational or due to the ordinary vicissitudes of advancing age.

There is one point upon which all the great minds seem to agree; that is, noise *does* fatigue. Standing a watch over a couple of laboring Diesel engines takes

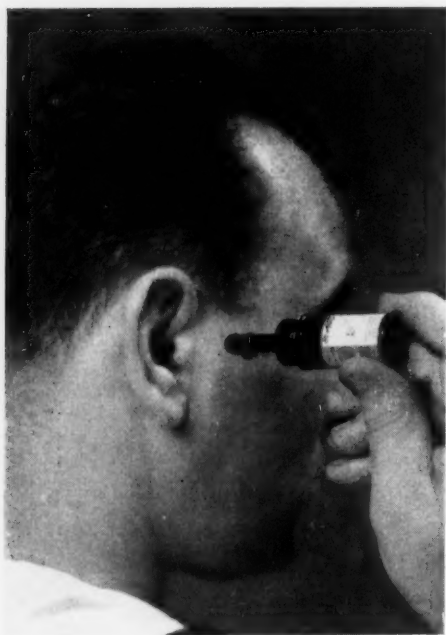


U. S. Navy Photo

THE V-51R EAR WARDEN

more out of a man than standing watch over a pair of purring turbines. The continued rat-a-tat of a riveting machine in an aircraft factory raises the curve of absenteeism, although no notice has been taken of this saboteur.

The Navy's answer to this problem has been cotton—wads of it. It is said that the Union admirals were considerably disturbed during the Civil War over the necessity of using cotton before starting a shore bombardment problem. But that is the only period when there was any doubt about using cotton. For generations it has been the universal panacea against noise and blast. It must be admitted in deference to the Cotton Growers Association that cotton



U. S. Navy Photo
INSERTING WARDEN

properly inserted in the ear canal offers good protection against blast and noise.

As the tempo of the recent war increased, and the noise level on ships likewise, the Navy sought the best ear protection obtainable. Was it cotton or was it some mechanical device? Of the making of earplugs there is no end. Some are good, some are bad, and some are indifferent. Before a suitable ear protective device could be selected for universal naval issue, it had to meet certain basic requirements.

The first and most important requirement was that the device would have to offer greater protection than cotton. There would be no point in introducing a mechanical device that would not do the job better than cotton.

The next consideration was that of comfort, for any safety device must be reasonably comfortable or men will not use it. Safety engineers sometimes lose sight of this fact. Men can be ordered to use ear protection or goggles or any-

thing else, but unless they are comfortable they will find an infinite number of ways for evading their use.

It also had to be borne in mind that practically everyone on board ship from the captain to the cook at some time or other must wear the "phones." This introduces two separate and distinct problems. The ear protectors must not interfere with the use of the phones and, while excluding noise, they must not reduce the possibility of hearing commands and orders—requirements that appear to be contradictory and impossible to meet.

Finally, as the bulk of the fleet operations were being conducted in the tropics, there was the ever-present danger of fungus growth, which has been described as "athlete's foot" of the ear. Would the introduction of a mechanical device into the ear increase the possibility of infection?

All too often when faced with a problem that contained so many unknowns in the equation, it has been necessary for the Navy Department to institute a program of research. After the research has been conducted and the data analyzed, a laboratory device must be designed and submitted to exhaustive tests. Finally, the laboratory model has to be transmuted into a production model. This takes time, valuable time.

In the matter of ear devices, time had been seized by the forelock. Research on ear protective devices had been going forward for a number of years. Dr. Verne Knudson, of the University of California, aided by a number of able assistants, had collected data on ear protectors and embodied the result of these researches into a number of fundamental designs. The Psycho-Acoustic Laboratory, Harvard University, working under a contract from the National Defense Research Committee, continued Knudson's investigations. At the same time, it conducted a program of comparative

tests of such ear protectors as were commercially available. The sum total of all research was embodied in an ear protector known as the V-51R Ear Warden. Upon the basis of objective tests, this device seemed to be the nearest approach to a solution of the problem.

Test data definitely indicated that over the frequency range of audible sound waves the V-51R Ear Warden, in addition to being superior to other mechanical devices tested, offered greater protection than cotton. Further, under conditions of high noise level, intelligibility of speech over the sound-powered phones was increased when the ear wardens were used. These two basic conclusions were enough to warrant the procurement of the device for naval issue.

The possibility of infection of the ear canal by the introduction of the wardens was recognized. It was not certain, however, whether cotton or the wardens offered the greater hazard. Although cotton was antiseptic, the fingers that



U. S. Navy Photo

REMOVING WARDEN

pulled it out of a container and stuffed it into the ear were not. The wardens could be sterilized without too much difficulty in an antiseptic solution.

Neoprene was selected as the material from which the ear wardens were to be molded. In addition to meeting the technical requirements, it was as nearly nontoxic as any other available material. People whose skin was irritated by rubber were not, in the great majority of cases, disturbed by neoprene. With an eye cocked toward the requirement of comfort, the designers insisted upon a degree of softness in the material that caused the manufacturers to writhe in anguish. They said it couldn't be done, but, to their everlasting credit, it was.

It is interesting to note that once the laboratory produced ear wardens that satisfied all interested parties and the specifications for their production were approved, it required eight months before the wardens started to roll off the production line. The over-all time from the decision to purchase large quantities for service issue to their actual commer-



U. S. Navy Photo

WARDEN IN PLACE

cial production was approximately a year. There is no telling how long it would have taken to complete the basic research, if it had not been started before the war. These facts are introduced to buttress the argument for continuous research. Problems must be foreseen and the basic research conducted upon them long before the problem is recognized generally in the Fleet.

Ear wardens are now being given to the Fleet as general issue. As the tempo of production increases, they will have universal distribution. They should be used by all men exposed to gunfire or continuous high noise levels. Under such conditions they should be viewed in exactly the same light as a pair of goggles in a grinding operation. They represent a comfortable, usable safety device.

One thing remains to be done. We are now issuing devices to protect the hearing. We must now develop a device, or modify for service use an existing device, to test adequately the hearing of naval personnel. These tests should be conducted before a man is inducted and again before he is discharged. The click of a coin is a welcome sound in the pocket but a travesty as a test of hearing.

LT. COMDR. GEORGE W. DYSON

THE USE OF ATOMIC TRAIL BLAZERS IN PHYSIOLOGICAL INVESTIGATIONS WITH PLANTS

ONE of the greatest triumphs of mind over matter has been the discovery of methods of artificially transmuting one kind of atom into another. The atomic bomb is the most spectacular development from researches in this field, but other applications of this modern brand of alchemy hold greater promise for the welfare of man. Modern techniques of atomic physics make it possible to label certain molecules in such a way that they can subsequently be distinguished from other molecules of the same kind.

The use of such identifiable molecules finds a number of important applications in biology and medicine.

The commonest method of tagging molecules is by incorporating into them atoms which have been rendered artificially radioactive. Such radioactive *isotopes* (isotopes of an element are different varieties of atoms of that element having different masses but the same nuclear charge) of many of the elements can be prepared by bombardment of the atomic nuclei in a cyclotron and in other ways. A radioactive isotope is identical in its chemical properties with the commoner isotopes of the same element, which for most elements are nonradioactive. A radioactive isotope of any element betrays its presence wherever it may be by the continual emission of radiations or charged particles which can be detected with suitable instruments. Thus it is possible to trace such molecules, after ingestion or absorption, through an organism and often even to determine the chemical reactions in which they participate. This has never been possible by ordinary methods of chemical analysis because by such methods it is impossible to distinguish between the introduced molecules and others of the same species which were already present in the organism.

The radioactive tracer technique has been successfully employed in a number of investigations of medical interest. Such representative problems as the accumulation of iodine in the thyroid gland, the phosphorus metabolism of the human body, and the role of iron in the synthesis of hemoglobin have all been investigated by this method. Important advances in our knowledge of the physiology of other animals, plants, and microorganisms have also been made with the help of radioactive tracers.

A natural application of the tracer technique is to the problem of the movement of substances in plants. The prin-

cial tissues of translocation in plants are the xylem and the phloem, corresponding, in the stems of woody plants, to the wood and the inner bark, respectively. It has long been recognized that upward movement of water in plants occurs through the xylem (in woody plants only through the outer layers) and that downward movement of the foods synthesized in the leaves occurs in the phloem. Regarding the route traversed through the plant by the mineral salts absorbed from the soil there has been much less unanimity of opinion. Results of some investigators seem to indicate the phloem, of others the xylem, as the tissue through which most transport of mineral salts takes place from the roots to the aerial organs.

The use of radioactive compounds of sodium, potassium, and phosphorus has permitted a critical decision to be made between these two viewpoints. When willow and geranium plants were allowed to absorb such compounds, the results show unquestionably that their upward movement occurred in the xylem. No evidence was found of any upward movement in the phloem, although considerable lateral movement of the salts from the xylem to the phloem did take place.

In other experiments on the translocation of salts in plants the effect of injecting leaves with a radioactive phosphorus compound was tried. The molecules of this substance were found to be translocated out of the leaves and in the downward direction through the phloem, which is continuous from the leaves into the stem. Once in the stem a large proportion of the molecules moved laterally from the phloem into the xylem, in which they reascended the stem. These results suggest the occurrence of a regular circulation of at least some of the mineral salts in plants. Apparently most of the mineral salts absorbed by the roots of plants are translocated directly to the

leaves. Some of the molecules of a salt may be retained in a leaf or used metabolically in the leaf cells, particularly if it is in an early stage of growth. Other molecules of the same salt may move out of the leaf through the phloem and eventually back into the same or other leaves through the xylem. A given molecule may repeat this translocation cycle several to many times before actually becoming immobilized in a cell.

Not only have the movements of mineral salts within plants been studied by this method, but knowledge regarding the mechanism of ionic exchanges between the roots and soil has been advanced by applications of this technique. A two-way movement of potassium ions between roots and soil has been demonstrated by this method. Radioactive potassium has been shown to move from the roots of barley plants into the surrounding medium at the same time that nonradioactive potassium was moving from the medium into the roots. These results indicate that a constant exchange of potassium ions in the medium with potassium ions in the roots is in progress, even when no net absorption of the ions by the roots is occurring.

Photosynthesis is another process which has been studied with scientific profit with the aid of tagged molecules. This is the process in which carbohydrates are synthesized in green plants from carbon dioxide and water with the accompanying release of oxygen. Photosynthesis occurs only in light which furnishes the necessary energy. The entire world of plants and animals operates at the expense of the energy and organic capital accumulated in photosynthesis. Although this reaction runs smoothly in any green leaf exposed to proper conditions, man has never been able to duplicate it in the laboratory, and the essential mechanism of the process has thus far eluded all experimental probings.

Recent experiments with radioactive

carbon have thrown a new and different light on the probable mechanism of photosynthesis. When green plants were allowed to absorb carbon dioxide made with artificially radioactive carbon, it was possible, by suitable chemical analyses, to determine the kinds of chemical compounds into which the tagged carbon atoms were incorporated. The results of these investigations indicate that the first step in photosynthesis—which may take place in the dark as well as in the light—is the conversion of the absorbed carbon dioxide into carboxyl ($-\text{COOH}$) groups attached to molecules of very large molecular weight. This represents a considerable departure from most previous theories of the mechanism of photosynthesis. One tenacious, but never well-founded, theory of photosynthesis has been that formaldehyde is an intermediate product in the synthesis of carbohydrates. No evidence could be found in this investigation that any of the tagged carbon was present in molecules of formaldehyde or similar compounds, and hence no evidence in favor of this theory could be found by this technique.

For some elements there are several isotopes which can be used as tracers in physiological investigations. Carbon is an example of such an element. The usual variety of carbon atom has an atomic weight of 12, but isotopes with atomic weights of 11, 13, and 14 are also known. The unstable, radioactive C-11

isotope is the one which was used in the previously described investigations on photosynthesis, but presents some difficulties in experimental work because its radioactivity is not retained very long. The C-14 isotope is also radioactive, but thus far has not been much used because it is a weak emitter of charged particles and therefore difficult to detect. C-13 is a stable, nonradioactive isotope, but by a modification in technique such isotopes can also be used as tracers. Their presence is detected, not by their emission of radiations or charged particles, but by their larger mass as compared with ordinary carbon. Such determinations can be made with an instrument known as a mass spectrograph. The distribution of the recently made photosynthate throughout bean plants has recently been traced by allowing the leaves to absorb carbon dioxide containing the C-13 isotope. Rapid translocation of carbohydrates synthesized in photosynthesis to the growing stem and root tips was readily demonstrated by this method. Translocation of the carbohydrates, whether in the upward or downward direction, was found to occur in the phloem, thus confirming, with an entirely new technique, the generally accepted idea of the pathway of transport of carbohydrates in plants.

B. S. MEYER

DEPARTMENT OF BOTANY
THE OHIO STATE UNIVERSITY

COMMENTS AND CRITICISMS

Armchair Geology

I'm always glad to meet an old friend, and by the time I was two-thirds of the way through Mr. Chapman Grant's letter, in your January number, I knew I had found one.

A little over twenty-five years ago J. C. Branner, a geologist working on the Brazilian coast suggested the same hypothesis for the formation of beach cusps that Mr. Grant proposes. Branner's views are set forth in an article in the *Journal of Geology*, Volume 8, pages 481-484, for the year 1900. The idea received some credence for a time, especially among "armchair" geologists. It is an enticing hypothesis but it won't work. If Mr. Grant will check up by finding a smooth sand beach without cusps where cross-waves are coming in, I think he will see why.

Since the publication of my article, "Scientific Beachcombing," I have been watching each number of *THE SCIENTIFIC MONTHLY* for some criticism, from your readers, of the use of the method. So far the result is zero. I was, of course, interested in the use of the subjective method by Paul D. Harwood. The biography of John Ericsson is a "gem," but that is what we always expect from Owen Johnson.

O. F. EVANS

Oil, Water, and Vino

Dr. Adolph Knopf's article on strategic mineral supplies was fascinating and called attention to the magnificent work of geologists and engineers in exploration and inventory during the war.

However, he overlooked that group of geologists and engineers who find and produce the most valuable mineral of all, namely, water.

If national petroleum is a four-billion-a-year industry, national water is about an eight-billion-a-year industry. Overseas, the American soldier required more than 100 gallons of purified water a week (compared to 50 for petroleum products).

During the war huge water supplies had to be found and developed for hundreds of Army camps, thousands of industries, and many new and expanding cities and towns. Consulting water engineers, geologists, drillers, and chemists were strained to the utmost, while the Water Resources Branch of the Geological Survey, U. S. Department of Interior, answered thousands of water inquiries, and prepared plans for water supply in cooperation with the Army in all theaters of war.

Overseas, water production engineers outnumbered petroleum distribution engineers, and their mission was as vital. Rotary and cable tool well-drilling equipment was in continual use in search for underground water. As soldiers, too, water geologists fulfilled their mission of finding the "strategic mineral."

Water geology is too often overlooked by economic geologists when they consider the minerals of the earth. With all due respect to Dr. Knopf for his splendid article, he forgot the most important, and at times most strategic (e.g. El Alamein), mineral of them all.

I humbly quote the immortal words of Capt. William C. Rasmussen, water operations engineer of the Italian campaign, who has said: "There are times when a gallon of water is almost worth a bottle of vino."

WILLIAM C. RASMUSSEN

Zionism

For nine years I have looked in vain for an article on Zionism in *THE SCIENTIFIC MONTHLY*, so it is with great disappointment that I had to read the anti-Zionist statements masked under the title "The Social Significance of Jewish-Christian Inter-marriage" by George Wolff.

It is with difficulty that I restrain resultant emotions on the subject to write you at this time. I recall also having written to you several years previously requesting publication of some reference to the achievements of the modern Jewish renaissance in Palestine.

Evidently the front-page position of Palestinian news to-day prompted your printing the above-mentioned article. Unfortunately it expresses the opinions of the minority among Jews as well as Americans. (The Roper Poll recently showed American Jews to be 81 percent pro-Zionist, and the passage by both Houses of Congress of the Zionist Resolution for a Jewish Commonwealth with overwhelming majorities, proves how the American people as a whole insists its elected representatives vote.)

G. W. would have his reader believe that Zionism longs for "a new Jewish theocracy in Palestine." Nothing could be further from the actuality, or even the hopes of the religious wing among Zionists (Mizrachi). On the contrary, "Zionism is the expression of the national will to live of the Jewish people." (Quoted first sentence in the first chapter of *Zionist Education in the U. S.*, a survey by Samuel Dinin, Ph. D., published in October 1944 by the Education Committee of the Zion-

ist Organization of America, many of whose leaders are Rabbis.)

The author of the above-mentioned article insists that a "Jewish State would simply mean a glorified ghetto, narrow, undemocratic, and may well turn out to be reactionary." Such criticism is pure poppycock compared to the position of Zionist Palestine today. After much insistence on the part of Palestinian and world Jewry, reactionary colonial Britain allowed Palestine to present a Jewish Brigade against the tottering Nazis in the last days of the recent war. "Undemocratic" Jewish Palestine had 21 parties represented in the fall, 1944, elections. "Narrow" Jewish Palestine provided one of the chief strong points in the Near East of the Allies. The Hebrew University gave courses of advanced training for Allied Medical Officers in that theater. Jewish farms were an important source of fresh fruits, vegetables, and dairy products for Allied military personnel in the Near East.

Any GI who spent time in the rest centers near Tel Aviv looked on that spot as being of the widest horizon in that part of the world.

G. W. says "Nationalism has always and everywhere narrowed the mind." Does he think such is the case in American Nationalism? In Palestine the Jews are educating the Arab masses to progressive living (to wit: decreased Arab death rate, especially infantile, and increased Arab wages and standards of living). This latter activity has been the cause of feudal Arabian leaders' opposition to Zionism.

I wish he would have at least finished writing with the sentence "a permanent solution can only be found on the basis of an international covenant of nations." However, the irony of the situation that presents itself even now in the UNO is that the Jews, because they have not a few acres of soil they are free to call their own, cannot say a word in their own defense, or for their own welfare.

I. M. SATUREN, V.M.D.

Three Jeers for the Editor

It seems to me that you have done no service to THE SCIENTIFIC MONTHLY or the A. A. A. S. in publishing Owen Johnson's article on John Ericsson in your January issue. The MONTHLY ordinarily follows such standards of accuracy and interest that this aberration calls for comment.

I base my objection to the article on two general grounds: 1—The source; 2—The content.

1. *The Source.* It appears clearly below the ideals of the MONTHLY to accept material from a publication of such dubious integrity as the

present *Reader's Digest*. At least it is to the credit of the MONTHLY to have acknowledged the source.

2. *The Content.* The article is an uncritical sanctification of Ericsson in the worst tradition of romantic biography. That, however, may be called a matter of taste and therefore only my personal opinion. So let us turn to some of Mr. Johnson's factual statements and implications and see whether they are appropriate to a scientific journal.

Take for a starter page 15, col. 2, where Johnson speaks of Robert Fulton "little dreaming that a genius was born who would nullify all that he was creating" (emphasis added). Comment on this leap of fancy seems superfluous.

On page 16, col. 1, Johnson speaks of "poverty, the greatest spur of all." I wonder how many will agree with that characterization, having in mind, let us say, Darwin and Mareoni?

On page 18, col. 1, the author says of the paddle-wheel steamer, "but even under favorable circumstances it could develop a speed of only four to six knots." Johnson was born in New York; did he never see the old side-wheeler *Mary Powell* which for nearly sixty years reeled off sixteen to twenty knots on the Hudson?

On the same page, col. 2, Johnson discusses the Francis B. Ogden, Ericsson's screw steamer. Of the Admiralty he says, "They had listened to the engineer corps of the nation, which was arrayed *unanimously* against this ridiculous invention" (emphasis added). How does he know the opinion was unanimous? It seems unlikely, and if true, authority should have been given. Further on he says the Ogden smoothly and effortlessly moved . . . "without in the least shaking their convictions," and, "Not one had the slightest suspicion that he had taken part in the first successful demonstration . . ." (emphasis added). "A second time Ericsson had stood on the brink of fame and seen it denied him." Aside from the questionable use of superlatives here which have doubtful factual justification, the whole story is weighted in a sense with which at least one authority disagrees. The *Encyclopedia Britannica*, 1944, vol. 8, pp. 684-5 says:

"In 1836 he took out a patent for a screw-propeller, and though the priority of his invention could not be maintained, he was afterwards awarded a one-fifth share of the £20,000 given by the Admiralty for it. At this time Capt. Stockton, of the U. S. Navy, gave an order for a small iron vessel to be built by Laird of Birkenhead, and to be fitted by Ericsson with engines and a screw. This vessel reached New York in May 1839."

Nothing about these details in Mr. Johnson's article, but they quite alter the picture, don't they? Of course Mr. Johnson may be right and the *Britannica* wrong; but in that case at least a sentence of discussion seems required.

And finally Mr. Johnson says nothing about Eriesson's obstinacy in persisting with the colossal failure of the *Caloric*, a vessel powered with hot-air or "caloric" engines. She was built in defiance of sound theory and was a total loss on, I believe, her maiden voyage.

This letter is not intended as an attack on Mr. Johnson and still less on Eriesson, whose inventive successes are sufficient to maintain his reputation without suppression or coloring of fact. It is an attack on lending the support of *THE SCIENTIFIC MONTHLY* to a faulty and misleading biographical article that will doubtless be broadcast over the world by *Reader's Digest* as *THE SCIENTIFIC MONTHLY*'S.

In the above discussion I have selected only a few passages to illustrate my point. Many more could be cited and the discussion lengthened. But it hardly seemed worthwhile.

MARSTON L. HAMLIN

I am writing to comment on the short biography of John Eriesson which appeared in the January issue of *THE SCIENTIFIC MONTHLY*. It seems to me to be of a poor quality that does not often appear in the magazine, and if it represents the best efforts of a novelist there is a clear mandate to stick to scientific men as your contributors.

The exhortative tone is hardly suitable, and the bits of drama, the references to great but undemonstrated influences on subsequent history, are neither one typical of *SCIENTIFIC MONTHLY* articles. The paragraphs at the end of page 15 and continuing on page 16 exemplify these objectionable attitudes.

Moreover, the policy of accepting articles from the digest magazines does not seem appropriate to a scientific periodical even if they were of higher quality than this one. I should prefer to pay somewhat higher dues in order to be assured of the independence of the periodical from outside organizations.

If publication of the Eriesson article causes other reader comment, I would appreciate seeing a consensus of it on your editorial page.

DAVID H. MILLER

In "Meet the Authors," during the comment on Owen Johnson, it is suggested that *Reader's Digest* planted the article. Is it a policy of *THE SCIENTIFIC MONTHLY* to accept RD plants or is this the first occasion?

Since so many people, I think justifiably,

look with deep suspicion on any material emanating from or "republished" by RD, I wonder if such articles should not be so labelled at their heading so that readers who wish to may apply their customary grain of salt where it seems called for. Perhaps SM does not make a policy of accepting plants which would seem to require such treatment, but RD is reputedly famous for slipping such material into seemingly innocuous articles on the bees and flowers.

What worries me is that should SM publish RD plants unannounced, it might cause readers to lose respect for or faith in the integrity of the whole magazine as being a stooge for *Reader's Digest*. That would be a great pity and quite undeserved.

AUSTIN W. MORRILL, JR.

O. F. Evans appears to be the only Owen Johnson fan among those who commented on "John Eriesson." The antagonists objected to the article on two counts: its lack of scientific accuracy and restraint; and its origin, from the *Reader's Digest*. The first objection is sound, and I plead guilty of having accepted a manuscript from a nonscientific source without checking the validity of the facts and interpretations given. As a rule we can rely on the accuracy of our articles, because most of them are written by scientists. One might expect a novelist to be more concerned with dramatic effects than with plodding facts, and it was for the drama of Johnson's article that I accepted it, hoping that the facts had been treated with respect. I have contended, and still assert, that it is possible for scientists to inject drama into their stories without violation of facts. I hoped that the article on Eriesson might encourage scientists to experiment with a more attractive style of writing than they habitually use, exemplified recently by O. F. Evans on beach cusps and Paul D. Harwood on phenothiazine.

The implication that there is something sinister in accepting an article offered by the *Reader's Digest* seems unjustified. Like any other manuscript that we consider, the article on Eriesson was offered gratis. I was free to accept or reject it. In answer to reader Morrill's question, this was my first occasion. If there should be a second, I will, if necessary ask for the opinions of appropriate advisers.—Ed.

THE BROWNSTONE TOWER



At the forthcoming meetings of the A. A. A. S. in St. Louis, March 27-30, 1946, professional science writers, representing the country's great newspapers and magazines, will report scientific de-

velopments as revealed by papers presented at the meetings. As a rule, these men and women do not get their information by listening to speakers as they appear on the program; they must depend largely on the contents of manuscripts or abstracts of manuscripts submitted by the authors to the Press Service of the Association prior to the opening of the meetings.

Although the information provided by scientists for the use of science writers has been improving in recent years both in quantity and in quality, we feel that many scientists still do not fully realize the importance of submitting information about their work to the press. They may think that their work is of no interest to the public or that the science writers are not capable of reporting it correctly. It is true, of course, that some investigations are of greater interest to the public than others, but no scientist should assume that his work is too recondite to have meaning to laymen if properly interpreted. He should submit the desired manuscripts or abstracts and let the science writers judge their news value. These writers are competent scientists who can and will report accurately those features of any investigation that seem to them to have significance to the public. They have a broader understanding of the advancing front of science than any specialist in research. It is largely through their reporting of scientific progress that public understanding and support of research are increased.*

Any scientist who is convinced that he owes

it to himself, his institution, and the public to make the results of his work available to science writers should then take the trouble to submit adequate information about his work. It is most helpful to send to the Office of the Association for the use of the Press Service two copies of each manuscript and two copies of an abstract of it. The abstract, if properly written, will enable the science writer to determine quickly whether the investigation should be reported; he can then turn to the manuscript for details, if needed. If copies of the manuscript cannot be provided, the abstract should be made to contain all essential information: the purpose of the work, the methods used, the results obtained, and the technical significance of the results. Science writers do not need or desire abstracts written down to an assumed layman's level. Technical terminology is familiar to them, and they prefer a thorough professional abstract to a scientific bedtime story. Science writers complain most bitterly of inadequate abstracts (summaries) which state only that certain problems or subjects were studied without telling exactly what was found out. Let it be resolved, therefore, to give the Press Service complete information.

In the SM comments on previously published articles cannot, as a rule, be included in the issue immediately following the articles in question. Consequently, readers may have forgotten the contents of these articles by the time criticisms of them appear. As readers cannot be expected to reread the older articles, it is desirable that comments should be intrinsically interesting and understandable without reference to the articles that gave rise to them. It might be desirable for commentators to summarize the points on which they are commenting.

In a few instances we have published short original articles in Comments and Criticisms to save space. We shall limit this practice and discontinue it if possible.

F. L. CAMPBELL

THE SCIENCE LIBRARY

A.A.A.S. MEETINGS, ST. LOUIS, MARCH 27-30, 1946

THE revival of annual meetings by the whole Association, after a lapse of nearly five years except for the meeting in Cleveland in 1941, brings with it the resumption of the Science Exhibition and one of its notable features, The Science Library. Scientists who have traveled the circuit of Association meetings in the past twenty years will well remember this part of the meetings; younger scientists who will be attending a meeting of the Association for the first time have a treat in store for them. Through the cooperation of book publishers, university presses, libraries, and foreign governments a collection of more than one thousand of the most important recent scientific books and publications has been secured and will be placed conveniently for examination. Practically every American publisher of scientific material is represented in The Science Library. With the help of various foreign embassies and information services, it is expected that there will be several hundred books from overseas. The inclusion of foreign books on so great a scale is an innovation which it is hoped will lead to even larger exhibits as the years and the meetings pass.

On the following pages there appears a complete list of the books submitted for The Science Library as this issue of the SM goes to press. Some publishers have not yet submitted the titles they expect to ship to St. Louis, and the time available after it became possible to hold a meeting has not been sufficient to receive new books from overseas. But they have been promised and are said to be on the way. It is expected that in the six weeks that will elapse between this writing and the opening of the

meeting in St. Louis all the necessary information about foreign books and publications will be available, and it will be incorporated in the reprints of the lists to be distributed at The Science Library.

The books are classified by major branches of science. In addition, each publisher has keyed his titles so that their level of use can easily be determined. The key letters appear at the end of the citation for each book and can be interpreted from the following explanation:

B: Biography.

GS: General Science.

J: Juvenile.

P: Popular.

R: Reference.

S: Study and Teaching.

TH: Textbooks for High Schools.

TU: Textbooks for University and College.

The Science Library will be open from 8:30 A.M. to 6:00 P.M. Through the cooperation of the local universities and the St. Louis Public Library trained librarians will be in attendance all day. Members who wish to order copies of books that they have inspected may do so by means of the publishers' order blanks, which will be forwarded to their offices at the end of each day.

The staff members of the Association who have worked on this project extend an invitation to every attending member and visitor to browse among this unusual collection of scientific books. The entire Science Library Exhibition will be located in the St. Louis Municipal Auditorium, and ample facilities will be available for an unhurried inspection of these books.

General

- BAITSELL, G. A. (ed.). *Science in Progress*. First Series. 322 pp. (1st ed.) 1939. \$4.00. Yale. R.
- BAITSELL, G. A. (ed.). *Science in Progress*. Second Series. 317 pp. (1st ed.) 1940. \$4.00. Yale. R.
- BAITSELL, G. A. (ed.). *Science in Progress*. Third Series. 322 pp. (1st ed.) 1942. \$3.00. Yale. R.
- BAITSELL, G. A. (ed.). *Science in Progress*. Fourth Series. 331 pp. (1st ed.) 1945. \$3.00. Yale. R. (Four volumes of *Science in Progress* sold as set. \$12.50.)
- BAWDEN. *Man's Physical Universe*, rev. 832 pp. \$4.00. Macmillan. TU.
- FLOHERTY, J. J. *Behind the Microphone*. 207 pp. (1st ed.) 1944. \$2.00. Lippincott. J.
- FLOHERTY, J. J. *Flowing Gold*. 256 pp. (1st ed.) 1945. \$2.50. Lippincott. P.
- FLOHERTY, J. J. *Inside the F.B.I.* 192 pp. (1st ed.) 1942. \$2.00. Lippincott. P.
- FURNAS, C. C. *Storehouse of Civilization*. 562 pp. (1st ed.) 1939. \$3.25. Columbia University. R.
- GILL, HENRY V. *Fact and Fiction in Modern Science*. viii plus 136 pp. (2nd printing) 1945. Reg. ed., cloth: \$2.50. Fordham University Press. P.
- GILL, HENRY V. *Fact and Fiction in Modern Science*. viii plus 136 pp. (2nd printing) 1945. College ed., paper: \$1.00. Fordham University Press. TU.
- GUBERLET, MURIEL. *Hermie's Trailer House*. 32 pp. (1st printing) 1945. \$1.25. Cattell. J.
- HARRISON, GEORGE RUSSELL. *How Things Work: Science for Young Americans*. 288 pp. (1st ed.) 1941. \$2.75. Morrow. J.
- HUNTINGTON, E. *Mainsprings of Civilization*. 660 pp. (1st ed.) 1945. \$4.75. Wiley. TU.
- ILIN, M. (pseud.). *Ring and a Riddle*. 70 pp. (1st ed.) 1944. \$2.00. Lippincott. J.
- ILIN, M. (pseud.). *100,000 Whys*. 137 pp. (1st ed.) 1933. \$1.60. Lippincott. J.
- Low. *Science Looks Ahead*. 640 pp. (1st ed.) 1942. \$4.50. Oxford. J.
- LUCAS, J. M. *Indian Harvest*. 120 pp. (1st ed.) 1945. \$2.00. Lippincott. J.
- MACNEIL. *Between Earth and Sky*. 64 pp. (1st ed.) 1944. \$1.50. Oxford. J.
- MCCONNELL, J. *Nurse, Please!* 18 pp. (1st ed.) 1944. \$1.00. Lippincott. P.
- MOORE, DOM, T. V. *Principles of Ethics*. 405 pp. (1st ed.) 1943. \$3.00. Lippincott. R.
- POLLACK, PHILLIP. *Careers in Science*. 222 pp. (1st ed.) 1945. \$2.75. E. P. Dutton. P.
- Science in Soviet Russia, A Symposium of Papers Presented at Congress of American-Soviet Friendship* New York City. 100 pp. (1st printing). 1944. \$1.50. Cattell.
- SELL. *Comprehensive English-Spanish Technical Dictionary*. 1400 pp. (1st ed.) 1944. \$30.00. McGraw-Hill.
- VAN NOSTRAND. *Scientific Encyclopedia*. 1234 pp. (1st ed.) 1938. \$10.00. Van Nostrand. R.
- YATES, R. F. *Machines over Men*. 250 pp. (1st ed.) 1939. \$2.00. Lippincott. R.
- Year Book No. 43*. 206 pp. 1944. \$1.00 paper; \$1.50 cloth. Carnegie Inst. R.
- YOST, E. *American Women of Science*. 232 pp. (1st ed.) 1943. \$2.00. Lippincott. B.
- YOST, E. *Modern Americans in Science and Invention*. 269 pp. (1st ed.) 1941. \$2.00. Lippincott. B.

Aeronautics

- BENHAM, H. E. *Aerial Navigation*. 344 pp. (1st ed.) 1945. \$4.00. Wiley. TU.
- FLOHERTY, J. J. *Aviation from Shop to Sky*. 209 pp. (1st ed.) 1941. \$2.00. Lippincott. J.
- GLAUERT. *Elements of Aerofoil and Airscrew Theory*. 228 pp. \$3.50. Macmillan. R.
- HADINGHAM, RONALD. *Astronomical Air Navigation*. 132 pp. 1944. \$2.50. Crowell. TU & R.
- KELLS, KERN, and BLAND. *Navigation*. 479 pp. (1st ed.) 1943. \$5.00. McGraw-Hill. TU.
- LIMING. *Practical Analytic Geometry with Applications to Aircraft*. 328 pp. \$4.50. Macmillan. TU.
- MUDGE. *Meteorology for Pilots*. 259 pp. (1st ed.) 1945. \$3.00. McGraw-Hill. TU.
- NIKOLSKY, ALEXANDER A. *Notes on Helicopter Design Theory*. 236 pp. (1st ed.) 1944. \$3.00. Princeton. R.
- PARKINSON. *Aerodynamics*. 112 pp. \$2.25. Macmillan. P.
- ROBERTS, HENRY W. *Aviation Radio*. 652 pp. (1st ed.) 1945. \$5.00. Morrow. P.
- SERRALES. *English-Spanish and Spanish-English Dictionary of Aviation Terms*. 131 pp. (1st ed.) 1944. \$2.50. McGraw-Hill.
- STEWART, NICHOLS, WALLING, and HILL. *Aircraft Navigation*. 146 pp. \$1.50. Macmillan. TU.

VETTER, ERNEST G. *Visibility Unlimited: An Introduction to the Science of Weather and the Art of Practical Flying*. 356 pp. Illus. (1st ed.) 1942. \$4.00. Morrow. P.

VON MISES. *Theory of Flight*. (1st ed.) 1945. \$6.00. McGraw-Hill. TU.

Agriculture

ANDERSON. *Introductory Animal Husbandry*. 777 pp. \$4.00. Macmillan. TU.

BARGER, E. H., and CARD, L. E. *Diseases and Parasites of Poultry*. 399 pp. (3rd ed.) 1943. \$3.75. Lea & Febiger. TU.

BAVER, L. D. *Soil Physics*. 370 pp. (1st ed.) 1940. \$4.00. Wiley. TU.

BAXTER, D. V. *Pathology in Forest Practice*. 627 pp. (1st ed.) 1943. \$5.50. Wiley. TU.

BIESTER, H. E., and DE VRIES, LOUIS, (ed.). *Diseases of Poultry*. 1,020 pp. (1943, 3rd printing 1945) \$8.50. Collegiate Press, Inc. T.

BLACK, JOHN D. *Food Enough*. 280 pp. (1st printing) 1943. \$2.50. Cattell. P.

BRANDT, KARL. *The Reconstruction of World Agriculture*. 416 pp. (1st ed.) 1945. \$4.00. Norton. P.

BRUNNER, E. DES. *Farmers of the World*. 208 pp. (1st ed.) 1945. \$2.50. Columbia. S.

BUNCE, ARTHUR C. *Economics of Soil Conservation*. 227 pp. (1st ed.) 1942. \$3.00. Collegiate Press, Inc. R.

CHANDLER, W. H. *Deciduous Orchards*. 438 pp. (1st ed.) 1942. \$4.50. Lea & Febiger. TU.

CLARKE. *The Study of the Soil in the Field*. 228 pp. (3rd ed.) 1941. \$2.25. Oxford. S.

ESPE, DWIGHT. *Secretion of Milk*. 350 pp. (1938, 3rd ed. 1945) \$3.25. Collegiate Press, Inc. T.

GRAHAM. *Natural Principles of Land Use*. 288 pp. (1st ed.) 1944. \$3.50. Oxford. P.

GUSTAFSON. *Soils & Soil Management*. 424 pp. (1st ed.) 1941. \$3.00. McGraw-Hill. TU.

HAMMER, B. W. *Dairy Bacteriology*. 482 pp. (2d ed.) 1938. \$5.00. Wiley. TU.

HOGNER. *Farm Animals*. 196 pp. (1st ed.) 1945. \$3.50. Oxford. J.

MILLAR, C. E., and TURK, L. M. *Fundamentals of Soil Science*. 462 pp. (1st ed.) 1943. \$3.75. Wiley. TU.

KRYNINE. *Soil Mechanics*. 451 pp. (1st ed.) 1941. \$5.00. McGraw-Hill. TU.

LUSH, JAY L. *Animal Breeding Plans*. 444 pp. (1937, 3rd ed. 1945) \$3.50. Collegiate Press, Inc. T.

LYON & BUCKMAN. *Nature and Properties of Soils*. 499 pp. (4th ed.) \$3.50. Macmillan. TU.

MARSHALL & HALNAN. *Physiology of Farm Animals*. 339 pp. (3rd ed.) \$4.50. Macmillan. R.

MERCHANT, I. A. *Veterinary Bacteriology*. 640 pp. (3rd ed.) 1945. \$6.50. Collegiate Press, Inc. T.

PERRY, ENOS J. *The Artificial Insemination of Farm Animals*. 266 pp. (1st ed.) 1945. \$3.50. Rutgers. S & P.

SCHULTZ. *Redirecting Farm Policy*. 75 pp. \$1.00. Macmillan. R.

VAN DERSAL. *The American Land: Its History and Uses*. 231 pp. (1st ed.) 1943. \$3.75. Oxford. P.

Anthropology

BOAS, FRANZ, and others. *General Anthropology*. 729 pp. (1st ed.) 1938. \$4.00. Heath. TU.

COLE-COOPER. *Peoples of Malay*. 362 pp. (1st ed.) 1945. \$4.00. Van Nostrand. R.

CRESSMAN, L. S., and collaborators. *Archaeological Researches in the Northern Great Basin*. 158 pp. 1943. \$3.00 paper; \$4.00 cloth. Carnegie Inst. GS.

DUBOIS, CORA. *The People of Alor*. 654 pp. (1st ed.) 1944. \$7.50. Univ. of Minnesota. R.

GRAY. *Man and His Physical World*. 665 pp. (1st ed.) 1942. \$3.75. Van Nostrand. TU.

ILIN, M. (pseud.). *How Man Became a Giant*. 265 pp. (1st ed.) 1942. \$2.00. Lippincott. J.

KENNEDY, R. *Bibliography of Indonesian Peoples and Cultures*. 212 pp. (1st ed.) 1945. \$2.50. Yale. R.

KIDDER, A. V., and RUPPERT, K., et al. *Contributions to American Anthropology and History Vol. VIII, Nos. 40 to 43*. 260 pp. 1944. \$3.50 paper; \$4.00 cloth. Carnegie Inst. GS.

LEIGHTON, ALEXANDER H. (Lt. Comdr. USNR, Medical Corps). *The Governing of Men*. 420 pp. (1st ed.) 1945. \$3.75. Princeton. P.

LINTON, RALPH. *Cultural Background of Personality*. 157 pp. (1st ed.) 1945. \$1.50. Appleton-Century. R.

LINTON, R. *The Science of Man in the World Crisis*. 520 pp. (1st ed.) 1945. \$4.00. Columbia. S.

LOWIE, R. H. *An Introduction to Cultural Anthropology*. 584 pp. 1940. \$4.00. Rinehart. TU.

MACCURDY, G. G. *Early Man*. 362 pp. (1st ed.) 1937. \$5.00. Lippincott. S.

- MALINOWSKI, B. *The Dynamics of Culture Change*. 171 pp. (1st ed.) 1945. \$2.50. Yale. R.
- MEAD, MARGARET. *From the South Seas: Studies of Adolescence and Sex in Primitive Societies*. 1088 pp. (1st ed.) 1939. \$4.00. Morrow. P.
- MONTAGU, M. F. ASHLEY. *Man's Most Dangerous Myth: The Fallacy of Race*. 304 pp. (2nd ed.) 1945. \$3.25. Columbia. S.
- MURDOCK, G. P., et al. *Outline of Cultural Materials*. 56 pp. (1st ed.) 1945. \$1.00. Yale. R.
- ROYS, RALPH L. *The Indian Background of Colonial Yucatan*. 244 pp. 1944. \$1.75 paper; \$2.75 cloth. Carnegie Inst. GS.
- RUPPERT, KARL, and DENISON, JR. JOHN H. *Archaeological Reconnaissance in Campeche, Quintana Roo, and Peten*. 156 pp. 1943. \$4.25 paper; \$4.75 cloth. Carnegie Inst. GS.
- VILLA, ALFONSO R. *The Maya of East Central Quintana Roo*. 182 pp. 1945. \$2.25 paper; \$2.75 cloth. Carnegie Inst. GS.
- FISHER, C., and LOCKWOOD, M. *Astronomy*. 205 pp. (1st ed.) 1940. \$2.25. Wiley. TU.
- GOLDBERG, L., and ALLER, L. H. *Atoms, Stars and Nebulae*. 323 pp. (1st ed.) 1943, reprinted 1945. \$3.00. Blakiston. S.
- MIEVILLE. *Astronomical Navigation Without Math*. 25 pp. \$.65. Macmillan. P.
- PANETH. *The Origin of Meteorites*. 27 pp. (1st ed.) 1940. \$.85. Oxford. S.
- SHAPLEY, H. *Galaxies*. 229 pp. (1st ed.) 1943, reprinted 1945. \$3.00. Blakiston. S.
- SHUTE, SHIRK, PORTER, and HEMENWAY. *Introduction to Navigation and Nautical Astronomy*. 457 pp. \$4.50. Macmillan. TU.
- SMART. *Text-book of Spherical Astronomy*, (4th ed.) 420 pp. \$4.75. Macmillan. TU.
- STARR, VICTOR P. *Basic Principles of Weather Forecasting*. 299 pp. (1st ed.) 1942. \$3.00. Harper. TU & R.
- WATSON, F. G. *Between the Planets*. 222 pp. (1st ed.) 1941, reprinted 1945. \$3.00. Blakiston. S.
- WHIPPLE, F. L. *Earth, Moon and Planets*. 293 pp. (1st ed.) 1941, reprinted 1945. \$3.00. Blakiston. S.

Astronomy

- BAKER. *Astronomy*. 527 pp. (3rd ed.) 1938. \$3.75. Van Nostrand. TU.
- BARTON and BARTON. *Guide to the Constellations*. 80 pp. (3rd ed.) 1943. \$3.00. McGraw-Hill. P.
- BEET. *A Text-book of Elementary Astronomy*. 110 pp. \$2.00. Macmillan. TU.
- BOK, B. J. and P. F. *The Milky Way*. 224 pp. (2nd ed.) 1945. \$3.00. Blakiston. S.
- CAMPBELL, L., and JACCHIA, L. *The Story of Variable Stars*. 226 pp. (1st ed.) 1941, reprinted 1945. \$3.00. Blakiston. S.
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